EA-87-02

Ontario

ENVIRONMENTAL ASSESSMENT BOARD

VOLUME: 122

DATE: Friday, August 11th, 1989

BEFORE: M.I. JEFFERY, Q.C., Chairman
E. MARTEL, Member
A. KOVEN, Member

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EA-87-02

HEARING ON THE PROPOSAL BY THE MINISTRY OF NATURAL RESOURCES FOR A CLASS ENVIRONMENTAL ASSESSMENT FOR TIMBER MANAGEMENT ON CROWN LANDS IN ONTARIO

IN THE MATTER of the Environmental Assessment Act, R.S.O. 1980, c.140;

- and -

IN THE MATTER of the Class Environmental Assessment for Timber Management on Crown Lands in Ontario;

- and -

IN THE MATTER OF a Notice by the Honourable Jim Bradley, Minister of the Environment, requiring the Environmental Assessment Board to hold a hearing with respect to a Class Environmental Assessment (No. NR-AA-30) of an undertaking by the Ministry of Natural Resources for the activity of timber management on Crown Lands in Ontario.

Hearing held at the Ramada Prince Arthur Hotel, 17 North Cumberland St., Thunder Bay, Ontario, on Friday, August 11th, 1989, commencing at 8:30 a.m.

VOLUME 122

BEFORE:

MR. MICHAEL I. JEFFERY, Q.C.	Chairman
MR. ELIE MARTEL	Member
MRS. ANNE KOVEN	Member

A P P E A R A N C E S

MR. V. FREIDIN, Q.C.) MS. C. BLASTORAH) MS. K. MURPHY) MS. Y. HERSCHER)	MINISTRY OF NATURAL RESOURCES
MR. B. CAMPBELL) MS. J. SEABORN)	MINISTRY OF ENVIRONMENT
MR. R. TUER, Q.C.) MR. R. COSMAN) MS. E. CRONK) MR. P.R. CASSIDY)	ONTARIO FOREST INDUSTRY ASSOCIATION and ONTARIO LUMBER MANUFACTURERS' ASSOCIATION
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MR. J. EBBS	ONTARIO PROFESSIONAL FORESTERS ASSOCIATION
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MR. D. COLBORNE	GRAND COUNCIL TREATY #3
MR. R. REILLY	ONTARIO METIS & ABORIGINAL ASSOCIATION
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MR. G.J. KINLIN	DEPARTMENT OF JUSTICE
MR. S.J. STEPINAC	MINISTRY OF NORTHERN DEVELOPMENT & MINES
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MR. P.D. McCUTCHEON	GEORGE NIXON
MR. C. BRUNETTA	NORTHWESTERN ONTARIO TOURISM ASSOCIATION

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---Upon commencing at 8:30 a.m.

THE CHAIRMAN: Thank you. Good morning.

MS. CRONK: Mr. Chairman, just before we begin, I have had some brief discussions this morning with Mr. Freidin about scheduling matters and perhaps it would be appropriate to raise it now.

I'm wondering if Mr. Freidin and Ms. Murphy are in a position to inform the Board and other counsel as to their best estimate today as to when their case might conclude, taking into account the Dryden satellite location hearing.

I have, of course, a number of reasons for raising that, given that our case is to commence immediately after the evidence of Dr. Baskerville which will follow the MNR's case, and perhaps I could tell you what the concerns are from our point of view after we hear about the scheduling projections from Mr. Freidin.

THE CHAIRMAN: Okay. Mr. Freidin?

MR. FREIDIN: Our best estimate is that we would complete our case on November the 15th. I can give you a brief idea of how I arrive at that.

It's primarily another week for this panel, three weeks of cross-examination on Panel 14, which is what the indication was before we broke;

starting Panel 15 on the 11th of September, taking a week perhaps in direct; a week of cross-examination; off to Dryden until the end of September; two more weeks of cross-examination on 15 which would take us to October the 15th; and then three weeks for Panels 16 and 17, both direct and cross-examination.

Giving myself a week leeway on that schedule, it takes us to November the 15th.

THE CHAIRMAN: And I guess there is no indication at this time how long Baskerville might be?

MR. FREIDIN: Only the comment made by Mr. Turkstra the other day that he thought that the examination could be a week to two in direct, as I recall his comment. Other than that, I have no idea how long it might take.

THE CHAIRMAN: And in December we will be rising prior to the 15th of December. When I say prior, not later than, until the first week in January when we could recommence. So it looks unlikely that we would finish Dean Baskerville before the break.

MR. FREIDIN: I think if we wanted to ensure that we have set enough time aside for him, I think you are right. But that's the best estimate that I can give, Mr. Chairman, and perhaps that would be helpful to Ms. Cronk and she could make her submissions

based on that information.

MS. CRONK: It was in part for those reasons that I raised it, Mr. Chairman. Obviously -- well, one of the first panels that our client -- will be presented on behalf of our clients is comprised of senior executives from a number of the industry companies.

Their schedule, like everyone else's, and the many experts who have appeared before you, are difficult to deal with. They are particularly difficult to deal with when they spend a great deal of time out of the country or in other locations in the country, so we were concerned about being able to assure that they were here when they were required to be.

When I knew the November 15th date that was being projected and then heard Mr. Turkstra indicate that the examination in direct of Dean Baskerville might take as long as one or two weeks, it occurred to me, sir, likely, although of course not probable, that our case would not in the normal course be called upon to start until the beginning of the new year.

It would be of enormous assistance to us in our preparation and as well to arranging the

scheduling of a large number of people, if the Board and other counsel are in a position to do so, to indicate that we could assume that we will not be starting until the first week of January.

I recognize that on occasion things have been abbreviated during the course of the evidence here so that things are moved up, but regrettably that's not historically the direction it has gone in.

So I would think that it might be an overage as opposed to underage, but you understand the problem, sir.

What I'm really saying is, that if we could obtain a fixed date, recognizing the best submissions you have heard, it would help enormously in the scheduling and also our ability to ensure that people whose schedules are most demanding are here when they are needed.

---Off the record discussion

MS. CRONK: Ms. Murphy was just pointing out as well, Mr. Chairman, the psychic as she sometimes is, that a fixed date assists in determining the time for delivering evidence packages.

THE CHAIRMAN: Well, Ms. Cronk, I think the Board can assist you in this way: Based on the best estimates it looks very much like we won't be able

to start with your case until the beginning of the new year realistically.

Should, by some fluke, history be rewritten so that we do in fact finish with Dean Baskerville earlier and there is some time that the Board would want to utilize, then perhaps we will give consideration to another satellite hearing.

And as long as we have enough notice period for that hearing - and we may be in a better position as we go down the track - we may be able to plug in another one at some stage if we have the available time.

Looking at the calendar, January the 1st comes out, I think it is on a Monday.

MR. MARTEL: Monday.

MR. CASSIDY: That's correct.

THE CHAIRMAN: And, consequently, it would be unlikely that we would start until the beginning of the following week. And you have chosen I believe to present your case in Thunder Bay; is that correct?

MS. CRONK: That's correct.

THE CHAIRMAN: So we would then recommence in the new year up here on the second Monday in January--

MS. CRONK: Thank you.

THE CHAIRMAN: --with your case.

MS. SEABORN: Mr. Chairman, would it be possible to consider starting on the Tuesday of that week? I only suggest that because we will not have been in Thunder Bay for some time and it will take ourselves at least some time to get organized and collect exhibits and set up our office, so to speak, for that portion of the case.

---Discussion off the record

THE CHAIRMAN: All right. The Board would consider starting on the Tuesday but sitting that Friday of that first week and that will give everyone an opportunity to get organized the first day back.

MS. CRONK: Thank you very much, Mr. Chairman. I take it then that the Board's direction to our clients is that our case will start no sooner than the Tuesday of the second week in January?

THE CHAIRMAN: That's correct. And you, of course, will be serving your witness statements within the--

MS. CRONK: Calculated time.

THE CHAIRMAN: --calculated time based on that; is that correct?

MS. CRONK: Yes. Thank you, sir. Thank

you very much.

THE CHAIRMAN: Okay.

PETER KINGSBURY,
LEONARD RITTER, Resumed

CONTINUED CROSS-EXAMINATION BY MS. CRONK:

Q. Dr. Ritter, I wonder if we might continue from where we were yesterday. You had before you when we were discussing Exhibit 716 an extract from the Crump risk assessment analysis on the aerial -- the risks of aerial spraying of pesticides.

We had a discussion yesterday towards the end of the day about the approach that had been used by the authors of this report and, to a certain extent, the methodology, you recall that at the end of the day yesterday?

DR. RITTER: A. Yes.

Q. Am I correct, Dr. Ritter, that the approach and the methodology to risk assessment adopted by the authors, as you described it yesterday, is one which would only overestimate risks and could not underestimate risks?

A. I think I would substitute your word only for most probably.

Q. All right. Thank you. Could I ask you then, if you would please, to turn back to page

(iv) of the executive summary that we were looking at yesterday. I understand what follows after the end of the discussion on page (iv) to be two summary tables and I would like to review those with you.

Just dealing with the first table, Table 1, which appears over on the next page, page (v), there is a description of the nature of the data contained in that table set out at the bottom of page (iv) and could I direct you to the last paragraph, the first part of which reads, the bottom of page (iv):

"The highest estimated risks from occupational exposures typically were for loaders whose job responsibility involves moving the herbicide from its container to the mix tanks and from there to the helicopter."

So stopping there for a moment, that means, as I understand it, that of all of the risks identified in the risk analysis for persons occupationally exposed to pesticides, those with the highest risks were loaders?

A. Yes, I read that the same way.

Q. All right. And then continuing with the next sentence:

"The following table..."

Referring to Table 1 on the next page:

"Gives margins of safety from worst-case estimates of exposure and worst-case estimates of lifetime risk of cancer to loaders from a single spray operation."

Stopping there. Do I understand that correctly, Dr. Ritter, to mean that what is set out in the table on the next page, first, is specific to the highest risk group; that is, loaders and; secondly, that the margins of safety are for the two worst-case situations; that is, the worst-case exposure and the worst-case estimates of lifetime risk of cancer?

A. I would read that the some way.

Q. All right. It's the worst on both accounts?

A. Yes.

Q. All right. Then finally in the same paragraph the authors continue to indicate:

"These expressions of risk represent the size of the population actually exposed according to the specific scenario, not the total population potentially exposed. For example, an estimated extra lifetime cancer risk of one in one million does not represent any population of one

million persons but rather one million persons actually exposed to that amount of specific herbicide under the specified exposure scenario."

Now, stopping there and dealing with what the authors are indicating. First, the extra lifetime cancer risk is the concept that you discussed with the Board yesterday?

A. That's correct.

Q. That is the approach taken in risk analysis of carcinogenic risk?

A. Yes, it is.

Q. And what significance do you attach to the indication that the authors have given that the population -- the expression of one in one million, represents those actually exposed to a specific pesticide as opposed to those potentially to be exposed?

What significance should we attach to that, if any? What do they say?

A. It is somewhat of a departure from the convention in doing these kinds of calculations, but I would think it would, again, have the tendency of exaggerating the risk estimate.

Q. What does it mean, though?

A. It means that in the calculation one has presumed that all of those people are exposed rather than simply potentially exposed, and that if they were all exposed this would be the excess risk. But if there is no assumption that there is any possibility of it, they might not be exposed.

Q. All right. Does that mean, for example then, before we turn to the actual numbers, that if the risk -- the quantification of the risk resulted in a number, for example, of .06, just for example, per million that that would mean that of one million persons actually exposed to cancer only six had an extra risk of actually suffering carcinogenic effects?

A. Not quite. It means if one million persons actually exposed, the excess risk in that group of one million would be .6 additional cancers, which is not quite what you said but similar to it.

Q. All right. Can we look then at the actual data and perhaps you can explain it further when we look at the numbers.

Table 1 on the next page, as I understand it, first of all, is the table setting out the worst-case for occupational exposures. This is the one dealing with loaders under the two worst-case

situations; is that correct?

A. Yes, it is.

Q. All right. And it deals both with non-carcinogenic risks and as well cancer risks?

A. Yes.

Q. All right. Could we look first at the non-carcinogenic risks. Am I correct that -- let's just look at the column so I am sure that we are understanding one another as we move through these tables. The first column identifies the nature of the herbicide?

A. Yes.

Q. The next three columns, I suggest, entitled -- with a caption above it entitled: Margins of Safety, pertain to non-carcinogenic effects?

A. That's correct.

Q. The last column deals with, as stated, cancer risks?

A. Yes.

Q. All right. Could we deal first then with the non-carcinogenic risks as set out in the table.

Would you agree with me -- and I am concerned only with 2,4-D and glyphosate, Dr. Ritter, those being the herbicides authorized for aerial spray

use in forestry in Ontario, so I am just looking at the first two chemicals.

For those risks identified for reproductive and teratogenic effects, would you agree with me that the margins of safety are all high for 2,4-D and glyphosate; they range from 137 to over a thousand?

A. I prefer your second suggestion, they range from 137 to over a thousand. As to whether or not they are high, I think is a matter of judgment.

Q. All right. The risk of the effect, would you agree, even for loaders, assuming these two worst-case assumptions; that is, the worse estimate of exposure, is low?

A. Yes.

Q. All right. And the margins of safety for systemic effects are lower than they are for reproductive and teratogenic effects, but they are at least five for 2,4-D and 139 for glyphosate?

A. That's correct, yes.

Q. And if we look at cancer risks, they are expressed per million?

A. Yes.

Q. Would you agree with me that the risks are negligible for both 2,4-D and glyphosate?

A. Yes.

Q. .04 for 2,4-D -- well, it ranges from .04 for 2,4-D to 9 per trillion for glyphosate; is that correct?

A. Yes.

Q. Could we turn now to the worst-case for environmental exposures as opposed to occupational exposures.

If I can stop here for a moment just to be clear. As I understand what the authors of this report did, they looked at a variety of potential risk situations or scenarios depending on, in the occupational context, the job function?

A. That's correct.

Q. Right. And in the environmental context that refers; does it not, to bystander or general public exposure risks?

A. That's right.

Q. And, again, the authors looked at a variety of potential risk situations under various exposure scenarios; is that correct?

A. That's correct.

Q. So that when we look at Table 1, as we just have with respect to occupational risk, what they are saying is of everything that we looked at for

those persons whose occupations may expose them to 2,4-D and glyphosate the category of person with the highest exposure -- with the highest risk were loaders and these are the results even for them?

A. That's right.

Q. All right. And then when we come to the environmental exposures, the summary table, Table 2 over on the next page deals again not with all of the scenarios that the authors considered in their analysis, but rather with the worst-case?

A. That's correct.

Q. Right. And if we -- could I direct you first to the bottom of page (v) which is the explanatory text for Table 2, being the summary table for environmental risks. The authors indicate:

"The scenario which gave the highest worst-case estimates of environmental exposures was eating 20 helpings of unwashed berries picked directly after a spray operation. The following table gives margins of safety from worst-case estimates of exposure and worst-case estimates of lifetime risk of cancer from eating berries."

Stopping there. Again, Dr. Ritter, if I

could seek your assistance.

As I understand it then, what the authors are indicating is that of all the risk scenarios in the environmental exposure category that were looked at, those which quantifiably had the highest degree of risk were for persons who ate 20 helpings of unwashed berries directly after a spray operation; is that correct?

A. I would read that the same way, yes.

Q. All right. And, once again, in setting out the summary data results they have taken the same approach as they did for the highest risk category of occupational exposures. They have indicated what the risk assessments were for those who had the worst exposures; that is, the most exposure, and the worst-case estimates of lifetime risk of cancer; is that correct?

A. Yes.

Q. All right. Could we go then directly to the table and could we deal again, first, with the non-carcinogenic effects and the risk of that. For 2,4-D and glyphosate they range from 23 to over 2,200; am I right?

A. Yes.

Q. They are all larger than the margins

of safety for occupational exposures that we just looked at in Table 1?

A. Yes.

Q. Does this mean, Dr. Ritter, that there is less chance of the health effects referred to in Table 2 occurring as a result of eating unwashed berries than from working as a loader in spray operations? Am I reading it fairly?

A. Yes.

Q. All right. And the margins of safety, I suggest, given that they range from 23 to over 2,000 are again very healthy; are they not? Perhaps that's a bad choice of words in the context.

A. They are larger than they are for occupational exposure.

Q. The risks of these effects actually being sustained is very low?

A. That's right.

Q. All right. In fact some might say negligible; would you agree?

A. Not necessarily. I think before one can attach the term negligible particularly to things like systemic effects it is important to understand what the nature of the effect is.

The margin of safety in itself when

quoted as an'absolute number doesn't have a great deal of meaning until one understands the particular end point to which that number is being modeled.

In other words, reproductive effects or teratogenic effects in itself is not very informative. There are many variables in a teratology study that are evaluated, some of significance and others which are relatively trivial.

Q. Can we fairly take, however, from this quantification - and let's take, for example, the teratogenic effects category - can we fairly take from it that it was the result of the analysis that potentially human health effects dealing with fertility or miscarriage were low?

A. Yes.

Q. All right. And similarly with respect to the reproductive effect category, such matters as birth -- risk of birth defects or malformations resulting from exposure to these two chemcials, those risks were low?

A. Yes.

Q. All right. Can we look then at the carcinogenic risk, the cancer risk on the right-hand side. They range, if I am reading the table correctly, from 9 per billion for glyphosate to 19 per million for

2,4-D. Am I reading it correctly?

A. That's right.

Q. All right. They are, I suggest, a very low negligible risk?

A. I'm sorry, I think you said 9 per billion, I think that's actually .9 per billion.

Q. Sorry, all right.

A. It is less than one per billion.

Q. I am sorry. So it's .9--

A. Yes.

Q. --per billion?

A. Yes.

Q. And is it to 19 per million, or is it .19?

A. No, it is .19.

Q. All right.

A. Or if you like 19 per billion rather than per million.

Q. All right. I suggest, Dr. Ritter, that those risks are indeed negligible; would you agree?

A. Yes, I would.

Q. All right.

THE CHAIRMAN: Dr. Ritter, how do you reconcile the statement above the tables saying:

"Those herbicides indicated by an asterisk have not been shown to be carcinogens."

And 2,4-D does not have an asterisk with the other study, the Ontario study, Exhibit 714?

DR. RITTER: Which indicated that 2,4-D was held not to be a carcinogen.

THE CHAIRMAN: Right.

DR. RITTER: I think you will find, Mr. Chairman, that that's on the basis of the dates during which these analyses were done. The Krump analysis was submitted in May of 1986 and the bulk of the work which went into this was actually conducted between 1984 and 1985.

At that time the information that was available on 2,4-D may have led some to conclude, at least in the absence of some clarification that came later on, that it was reasonable to assume that it was at least an animal carcinogen. Subsequent to that there were some additional analyses and investigations that were done on 2,4-D, and at about the time that the Ontario people were looking at it, I think most sources felt that the evidence was no longer sufficiently convincing to arrive at that conclusion.

THE CHAIRMAN: So if this were done

today, Exhibit 716 study, it would have an asterisk in front of it?

DR. RITTER: I think it probably would, yes.

THE CHAIRMAN: Thank you.

MS. CRONK: Thank you.

Q. I would like to come back to that answer you have given, Dr. Ritter, in the context of the World Health Organization report on 2,4-D that was done that you mentioned yesterday, but can you confirm to the Board that during the years 1984 to 1987 there was a considerable amount of continuing research done on the issue of the possible carcinogenic effect of 2,4-D?

DR. RITTER: A. Yes.

Q. All right. And so that a report written in 1984 based on the information database that existed at that time might, when viewed in today's light, not reflect the consensus of scientific community position on the issue that would pertain today?

A. Yes, I would agree with that. The two pivotal cancer studies I believe only became available in 1986 which would have been after the time that the Crump report was written.

Q. But before the time they were considered in the MOE report; were they not?

A. That's right. The MOE report would have considered and did consider in fact the two most recent cancer studies. The Crump Report I don't think could have.

Q. All right. And we will come to it later this morning, but with respect to the World Health Organization Report that you spoke about yesterday, first, that is specific to 2,4-D; is it not?

A. Yes, it is.

Q. Secondly, it was published in 1984?

A. Yes.

Q. And has, therefore, the same limitation that you have just mentioned with respect to the Crump Report?

A. Yes, it does.

Q. Thank you. Then if we could, just dealing with the cancer risk column for Table 2 before we leave it, the authors have attempted to put, as they describe it, those risks or those chances of cancer in perspective and they do so in the last sentence on page (vi) where they indicate:

"In order to put these estimates into perspective we note that the cancer risk

from smoking a single cigarette is about 0.6 per million; that from spending a day in the Rocky Mountains at an altitude of 10,000 feet from the extra radiation both cosmic and terrestrial is about 0.13 per million, and the extra risk of a fatal automobile accident from not wearing a seat belt for one day is about 0.3 per million."

Now, stopping there, Dr. Ritter, those are all activities which are not infrequently engaged in by some sectors of the general public; you would agree?

A. Yes.

Q. In fact they might even be engaged in in concert?

A. Yes.

Q. It seems, based on these examples - and I don't mean to be facetious in any way by this - that the highest risk of common, as an example, of common activities engaged in by the general public would be driving an automobile in the Rocky Mountains without your seat belt on and having a cigarette. You would agree?

A. Yes.

Q. All right.

THE CHAIRMAN: And hitting a truck.

MS. CRONK: I'm sorry?

THE CHAIRMAN: And hitting a truck.

MR. CRONK: Yes, and hitting a truck, that's right. That would take it perhaps off the map in terms of the degree of risk.

Q. But my point, in a serious way, is simply this, Dr. Ritter, that when we look at the degree of risk quantified in this study for cancer risks for either 2,4-D or glyphosate, they are, as you already indicated or agreed with me, very low, negligible, but in the context of risks to which members of the general public are routinely exposed they seem indeed very negligible; would you agree?

A. Yes.

Q. Thank you. Now, in both Table 1 and Table 2 the summary results for the worst-case risk of human health effects for picloram is also set out, but I should simply indicate to the Board and perhaps you can confirm in two parts:

First, the Board has received evidence and the evidence is that picloram is not authorized for aerial spraying in Ontario. So I ask you simply to accept that. Am I correct that this study was based on

aerial spraying conditions?

A. Yes, it was.

Q. All right.

MS. CRONK: And it is for that purpose, Mr. Chairman, that I won't be directing questions to the witness on the picloram results.

Q. Could I ask you to go then -- I would like to spend just a few moments, if we could, on the actual detailed results for both glyphosate and 2,4-D which are included in the extract forming this exhibit, and could we deal first with glyphosate, please.

There are four tables set out at pages 153 to 157 and they, as I understand it, are the depiction in tabular form of the results of the risk assessment for glyphosate; is that correct?

DR. RITTER: A. Yes.

Q. All right. The first three tables beginning at page 153, Table 5-5 and the next two tables deal with non-carcinogenic effects; is that correct?

A. That's correct.

Q. The fourth deals with cancer risks?

A. Yes.

Q. All right. So if we could again start with non-carcinogenic effects, Table 5.5, as I

understand it, deals with and sets out the margins of safety for non-carcinogenic systematic effects from exposure to glyphosate through a single spraying episode; correct?

A. That's correct.

Q. All right. Now, just stopping there for a moment. This table is constructed in somewhat of a different fashion from the summary tables that we just looked at and I would ask, if you could, simply to recall the discussion we had yesterday about the contents of the executive summary indicating the way the authors had approached this risk analysis.

Am I correct that what's set out in Table 5-5 are the quantification of risks which were the result of the risk assessment for both the worst-case risk scenario and, as well, what the authors termed to be a more reasonable risk scenario?

A. Yes, it is.

Q. All right. Not as extreme, more reasonable. All right.

Dealing then -- and that explains; does it not, the data set out under the more reasonable columns and the worst-case columns?

A. Yes.

Q. That's what they are?

A. Yes.

Q. And under each column the estimated human exposure is indicated and that is expressed in micrograms per kilogram of body weight per day?

A. That's correct.

Q. And then the margin of safety is indicated directly beside it?

A. Yes.

Q. And the risks quantified for both occupational exposures and environmental exposures are set out?

A. Yes.

Q. All right. Environmental again being bystander or general public exposure?

A. Yes.

Q. All right. Dealing then, first with the results for systematic effects, am I correct that the margins of safety under both the reasonable and the worst-case scenarios for glyphosate range from 138 through to 5,000?

A. Yes.

Q. Again, would you agree with me that the risks, based on those quantifications of actually suffering systematic effects, is low?

A. Yes.

Q. And if we look at what the authors have termed the more reasonable actual risk exposure situations, the margins of safety range from 555 to 5,000?

A. Yes.

Q. All right. And that's for occupational exposures. When we move into the environmental category, I suggest to you that the margins of safety are much, much higher and that, therefore, the degree of risk is much, much lower?

A. That's correct.

Q. And in fact, in some instances, we are talking about a margin of safety of 550,000?

A. That's correct.

Q. Could I ask you then to go to Table 5-6. Perhaps we could, as the format has now been discussed, go through these rather quickly. This table deals; does it not, with non-carcinogenic potential reproductive effects?

A. Yes, it does.

Q. Again, I suggest to you that in both the reasonable and the worst-case situations for both those occupationally exposed and those environmentally exposed the margins of safety are quite high?

A. Yes.

Q. In some cases extremely high?

A. Yes.

Q. The risk, therefore, of actually suffering these effects is low?

A. Yes.

Q. Table 5-7 dealing with non-carcinogenic teratogenic effects. Again, margins of safety in both the reasonable and the worst-case scenarios are very high; in some cases, extremely high?

A. Yes.

Q. Again the degree of risk, therefore, is very low?

A. Yes.

Q. And finally, if we could go to Table 5-8 dealing with cancer risks. On page 156, in this instance the quantification of risk is expressed differently and, as you explained yesterday, does not involve margins of safety per se?

A. That's correct.

Q. Is that correct? And as well, the mathematics are expressed in a different format than we have seen in the previous tables?

A. That's right.

Q. All right. Am I correct, however, that in looking at the quantification of risk; that is,

risk of cancer from exposure to glyphosate both in the occupational exposure context and in the environmental exposure context and both in the reasonable and worst-case scenarios, that the risks of cancer are very low indeed?

A. Yes.

Q. In fact negligible?

A. Yes.

Q. And could I take you to the comments of the authors regarding these tables at page 140 section under 5.3 dealing with risk characterization and this pertains, Dr. Ritter, to glyphosate.

The beginning of -- beginning with the second full paragraph on that page, do we find there the authors' comments on the results set out in the following tables that we have just reviewed?

A. Yes.

Q. And dealing first with the teratogenic effects indicated in Table 5-7 the authors suggest -- well, perhaps I should read it to you in fairness:

"All estimated margins of safety reported in Table 5-7 exceed 1,000 even in the worst-case, implying that the risk of teratogenic effects from exposure to

glyphosate through a single spraying episode is negligible if interpreted in terms of the traditional one hundred fold safety factor."

Do you agree with that interpretation of the results?

A. I agree with it, but in fact I think it's somewhat misleading. Glyphosate is actually not considered to be a teratogen.

Q. All right.

A. So that the statement here that implies that there is greater than a one thousand fold margin of safety for teratogenic effects leaves one with the impression that there are teratogenic effects to which this one thousand fold margin of safety exists.

Q. Right.

A. As I recall, that is not correct. The one thousand fold margin of safety is to a teratology study but not to teratogen effects per se.

The effects in that study which are being using to margin this model of safety are not based on birth defects, but rather to effects in the teratology study which I would certainly consider to be much less important that, frank, birth defects.

Q. All right. Well, thank you for that clarification then. And dealing with the authors' comments concerning the results pertaining to systematic or reproductive effects in the next two sentences of that paragraph, the authors suggest, and they describe, first of all, what the actual results were and they suggest that these results indicate that:

"With respect to non-carcinogenic risk the primary cause for concern, if any, from exposure to glyphosate is related to possible systematic effects. It should be recognized, however, that these margins of safety are conservative in the sense that they compare a single-day human exposures to average daily animal exposures over 90 days. In the worst-case scenario a worker would have to encounter 139 times the worst-case single-day exposure to reach a level equal to the NOEL, only above which are adverse effects considered to be likely."

They go on to indicate that:

"A margin of safety of 298 corresponding to the worst-case environmental scenario for exposure to glyphosate and that

298..." sorry.

"A margin of safety of 298 corresponding to the worst-case environmental scenario for exposure to glyphosate from eating wild berries means that an individual would have to consume approximately 160 pounds of contaminated wild berries in one day in order to reach an exposure equivalent to the NOEL for glyphosate."

Now, just dealing with those comments, Dr. Ritter. First, the conservatism that the authors suggest is that which we discussed yesterday; namely, that they have, for the purposes of their analysis, compared single-day human exposures to average daily animal exposures over 90 days?

A. That's correct.

Q. Correct. And would you agree with the implications of the authors' remarks that the degree of risk, when viewed in that perspective, is indeed quite low?

A. I would agree that when viewed in that perspective the risk is quite low. Exposure may, of course, in the human case take place for more than one day.

Q. Yes.

A. So I think they have -- in my view, they have somewhat over extended the value of that conservatism, if you like.

Q. All right. And I will come back with respect to all of these to the general issue of repeat exposure.

And the authors' discussion with respect to the significance of the cancer results for exposure to glyphosate are also set out at page 141 in the last two pararaphs. And, again, they indicate what the actual results were and they suggest that the estimated risk from occupational exposure to glyphosate through a single spraying episode range from 9 in 100-billion to 1 in 100-billion?

A. Yes.

Q. All right. And they further indicate and conclude on the basis of their analysis that the results suggest that the carcinogenic risk from exposure to glyphosate through a single spraying episode is negligible both for occupationally exposed and for environmentally exposed individuals?

A. Yes.

Q. Would you agree with that?

A. Yes, I would.

Q. All right. Could we look then next,

if we could please, at the results for 2,4-D and perhaps we will just do it from the back forward, so that we will deal first with the cancer risks, that is Table 4-8 at page 130.

And would you agree with me, looking at the risk assessment results for cancer risks as set out in this table, again both in the occupational and environmental exposure categories, and both under the more reasonable and the worst-case scenarios, that the results indicate negligible risks of cancer for persons in either category under either scenario?

A. Yes.

Q. And dealing with the teratogenic effects they are set out in Table 4-7. Would you agree with me that the results reported indicate large margins of safety under all the reasonable scenario situations and under most of the worst-case scenarios?

A. Yes.

Q. And extremely high margins of safety in many cases?

A. Yes.

Q. Dealing with the non -- suggesting that the risks of exposure -- the risks of sustaining effects of that kind are low?

A. Yes.

Q. And dealing with the non-carcinogenic reproductive effects, Table 4-5, there are similar results, similar -- I mean similar in the context to the ones that we have just considered for teratogenic effects; namely, there are very high margins of safety again?

A. You are referring to Table 4-6?

Q. Yes, I was. I beg your pardon.

A. Yes.

Q. And indeed in many cases they are very high; are they not?

A. Yes.

Q. Over 2-million in some instances?

A. Yes.

Q. Closer to 3-million in fact?

A. Yes.

Q. And then finally, dealing with Table 4-5, systematic effects, would you agree with me that the margins of safety for systematic effects are lower than with respect to the margins of safety for other non-carcinogenic effects?

A. Yes.

Q. And they are lowest under obviously the worst-case scenario?

A. Yes.

Q. And within that scenario they are the lowest for a loader where the margin of safety is 5.49?

A. That's correct.

Q. And that is the worst-case highest risk category dealt with in the summary table, Table 1, that we looked at?

A. Yes.

Q. All right. And the highest is 110,000 for ingestion of 2,4-D affected fish. Is that correct?

A. That's correct.

Q. Would you agree with me as well that in many cases the margin of safety is very high under both the worst-case and the more reasonable case scenario?

A. For environmental?

Q. Yes.

A. Yes.

Q. Yes. And even in the case of the lowest margin of safety, as the authors indicated, a worker would have to encounter 5.5 times the single-day exposure to reach a level equal to the NOEL; isn't that so?

A. That is what that says, yes.

Q. All right. Then dealing generally

with the implications of this study, Dr. Ritter, and bearing in mind what you told the Board both about the expertise and experience of Dr. Crump in this field and the nature of the available information in 1985 and '86 when this report was prepared, would you agree with me that based on this risk assessment report, in practical terms, there is negligible risk to humans of cancer afforded by exposure to 2,4-D or glyphosate?

A. Yes, I would.

Q. Would you also agree with me that in terms of non-carcinogenic effects this report again substantiates the view that there is negligible risk to humans of non-carcinogenic health effects by exposure to 2,4-D or glyphosate?

A. I think one might argue as to whether or not a margin of safety of six or five fold constitutes a negligible risk. With that qualification I would agree with you, yes.

Q. All right. And that qualification pertains to possible systematic effects for exposure to 2,4-D only?

A. That's right. But as I indicated, it's difficult to attach a value judgment to the significance of that margin of safety without an understanding of the end point to which that margin of

safety has been modelled.

Q. Do you have any concerns, based on the results in this report, Dr. Ritter, that there is a significant risk of human health -- negative human health effect as a result of exposure to 2,4-D or glyphosate?

A. No.

Q. Thank you. In either the occupational or the environmental exposure categories?

A. That's correct.

Q. That's correct, meaning you do not?

A. That's right.

Q. All right. And I said I would come back to the issue. Do you regard this report then as being reliable?

A. Yes, I do.

Q. All right. Could I ask you then to go back to the issue of more than single-day exposure and, as I said I would return to, and in that regard all of the tables that we have looked at pertain; do they not, to single-spray operations?

A. Yes.

Q. In terms of quantifying the degree of human risks?

A. That's right.

Q. However in arriving at the margin of safety, for example, in the non-carcinogenic effect category, single-day exposure for humans was compared, to arrive at the margin of safety, with multiple exposure situations based on animal tests; is that correct? 90-day exposure.

A. Not exclusively. Some of these end points that have been modelled are to teratology and reproduction and the principle of repeat dosing.

The teratology study, for example, is relatively meaningless. I can -- I don't know if that is worth pursuing or not, but the window of exposure necessary to induce a birth defect either in humans or experimentally is --

Q. Narrow.

A. --is relatively narrow.

Q. Yes.

A. Now, in the case of animal studies, that window is expanded somewhat to ensure that the precise moment in time when that birth defect can be induced is covered, so that one typically will cover a rather significant proportion of the gestational period just to cover off that possibility.

In reality we know that it takes a much narrower window of exposure. So that the principle of

the repeat exposure in something like a teratology study, for example, is relatively meaningless because it is not repeat exposure that is necessary to produce the effect, it was typically only a single day's exposure.

So, in that case, a single day's exposure is more directly referable to the nature of the protocol in the study than it would be, for example, in a 90-day study.

Q. All right. Am I correct, however, that where 90-day studies were done in accordance with appropriate protocol, it is the results under those 90-day studies that formed one of the benchmarks to arrive at the margin of safety for human exposure?

A. That's correct.

Q. All right. And I think I understand what you are saying with respect to the teratogenic studies, for example, simply being that there were some where there wasn't a 90-day situation because that wouldn't have been an appropriate way to proceed with testing in any event?

A. What I'm really saying is that the teratology study should really be seen as a one-day exposure study. Although exposure may have taken place for 10, or 11 or 12, it was only single day's exposure

that produced whatever effect has been seen.

Q. I understand. All right. Now, having said that, could I ask you to go, if you would, back to the text of the executive summary and, in particular, to page (viii).

And I direct your attention, Dr. Ritter, particularly to the second paragraph which deals, as I understand it, with the possibility of multiple exposures and the authors indicate:

"Synergism (antagonism) is said to occur when the effects from exposure to multiple chemicals are greater/less than expected."

Now, stopping there for a moment, can you distinguish for me, in this context, between synergism and antagonism; or are they the same?

A. They are not the same. Synergism is where the effects of two different chemicals given simultaneously would be the greater than the sum of either chemical alone. Antagonism is simply the converse of that.

Q. All right. So we are not talking about multiple exposure, we are talking about different compounds in that regard?

A. That's right.

Q. Then the authors continue:

"We feel that biologically significant synergistic or antagonistic responses are unlikely under the given exposure conditions for the herbicides being studied because exposures to different herbicides will generally be separated in time and because risks from exposure to each herbicide individually are estimated to be very small."

Now, stopping there for a moment. You might not be aware, Dr. Ritter, but this issue of possible synergistic effect has arisen through the course of other evidence before the Board.

Do you agree or disagree with the conclusion suggested by the authors in the sentence I have just read to you?

A. I neither agree nor disagree because I am not aware of the use, I am not that personally familiar with the use practice in Ontario.

If you are telling me that these chemicals are not used temporally, close enough in time so that one can really view it as a possible synergistic effect, then I'm prepared to take that at face value.

Q. All right. Would you agree with me that based on the risk analysis in this report, however, set aside the issue of actual use pattern in Ontario--

A. Mm-hmm.

Q. --and dealing just with the quantification of risks in this report, that the risks are in fact estimated in this report to be very small--

A. Yes.

Q. --based on the exposures dealt with in this report?

A. Yes.

Q. All right. And then, finally, the authors indicate:

"Consequently, risks from multiple exposures were estimated by assuming that Synergism or antagonism do not occur; i.e., by simply summing the risks from individual exposures."

A. Yes.

Q. Now, can you help me as to what that means in the context of the data we have looked at?

A. What the authors have done here is where there has been a calculation of multiple exposure, they have assumed that exposure has taken

place on several days to one chemical alone rather than to a series of chemicals.

Q. Thank you.

MRS. KOVEN: Excuse me, Dr. Ritter. Would they have also looked at a worker exposure and residential exposure and recreational exposure in that summing?

DR. RITTER: They may have. I can't really tell you precisely for any given instance immediately if they did or they did not, but if one were to look at environmental exposures, for example for most of the risks that have been investigated here, they are sufficiently small and I think it is very unlikely that they would make an important contribution to the overall calculation of risk.

When one sums a series of very small numbers, one still ends up with a small number.

MS. CRONK: Mrs. Koven, could I just ask you to repeat the three categories that you mentioned. It was worker exposure, residential and, I'm sorry, I missed the third.

MRS. KOVEN: And a recreational exposure.

MS. CRONK: Recreational. Thank you.

MRS. KOVEN: You know, the situation in northern Ontario in terms of use pattern is that there

are probably multiple sources of exposure, albeit small, one would think that the summing, if you were looking only in that northern Ontario experience - and the risk assessment wasn't done that way - that that would properly be part of it, I would think.

MR. RYDER: I think you are probably correct. I think to be absolutely precise one would do it that way, but on brief examination of the risks, as I have indicated, on brief examination of the risks or the exposures, whichever number one prefers, I would think it is unlikely to significantly alter the overall estimate because overall the estimates of exposure are extremely small.

MS. CRONK: Q. Thank you, Dr. Ritter. I would like to turn then very briefly to the World Health Organization study concerning 2,4-D that you mentioned yesterday.

Do you, by any chance, have a copy of it?

DR. RITTER: A. No, I don't.

Q. All right.

MS. CRONK: I should say, Mr. Chairman, for the information of the Board I don't propose to mark this but simply to refer very briefly to a number of sections given that Dr. Ritter mentioned it. I made you a copy. (handed)

THE CHAIRMAN: Thank you.

MS. CRONK: Q. Dr. Ritter, you have already confirmed for me that this report was released or available, as it were, in 1984; am I correct?

DR. RITTER: A. That's correct.

Q. And you indicated that it is particular as the title suggests to 2,4-D?

A. That's right.

Q. And as I understand, it is a document published under the joint sponsorship of the United Nations Environment Program, the International Labour Organization and the World Health Organization; is that correct?

A. That's correct.

Q. And you indicated yesterday that your laboratory was involved in the production of this report; is that correct?

A. We were much more than simply involved, we were requested by the World Health Organization to convene an expert panel to write this report and served as the secretariat for its preparation.

Q. All right. And did you then have responsibility for determining the list of experts described in the report who participated in it?

A. Through some process of consultation, yes.

Q. All right. And I think you have already confirmed for me that the conclusions expressed and the data reported upon in this document were based on the state of scientific information available in 1984, and I suppose prior to that when the work was being done?

A. That's correct.

Q. All right. Then, just dealing with it at that point in time, because the report has come up and it's been mentioned, I would like to refer you to the section dealing with the evaluation of health risks to man from exposure to 2,4-D; that is, Chapter 9 at page 99 very briefly, and I just ask for your confirmation as to certain of the conclusions or observations expressed in the report.

MS. CRONK: And I will simply put them on the record, Mr. Chairman, unless there is any need to have the pages marked.

Q. First, the first section in this chapter; am I correct, deals with general considerations relating to the evaluation of health risks to man from exposure to 2,4-D?

DR. RITTER: A. That's correct.

Q. And am I correct that it suggests, and I quote:

"In areas of 2,4-D herbicide production, handling or use, the highest exposure will be incurred by those who are directly involved in these processes followed by bystanders directly exposed to 2,4-D vapour, dust or droplets or to contaminated vegetation, soil, or water. In these two groups, exposure will usually be via the skin."

Now, stopping there for a moment. Do you agree that for the bystander group the dermal exposure is the significant feature -- factor?

A. It is a significant factor not only for bystander but indeed for occupational.

Q. All right. So that when we look at the tables from the Crump report in the environmental risk category that we have looked at, those dealing with exposure dermally become significant; do they not?

A. Absolutely.

Q. All right. And then again under general considerations the text indicates:

"The general population in 2,4-D use areas would be exposed to a lesser extent

mainly through food containing 2,4-D residues and to a lesser extent through 2,4-D residues in water. The contribution from air is negligible. As far as the general population is concerned 2,4-D intake from any source is negligible."

A. Yes.

Q. Do you agree with those observations, Dr. Ritter?

A. Yes.

Q. All right. And then dealing with the contents of the report regarding carcinogenic effects, could I direct your attention to page 101, Section 9.3.2.5. Am I correct that the authors indicate available animal bioassays and -- I'm sorry--

A. Epidemiological studies.

Q. Thank you. Are inadequate for an assessment of the carcinogenic potential of 2,4-D or of its derivatives. That is what the report indicates?

A. Yes.

Q. All right. Am I correct, Dr. Ritter, that that was a conclusion clearly reached at the time the report was prepared and not taking into account the work that became available to the MOE expert panel and

indeed, at least in some part, to the Crump authors?

A. Yes.

Q. Thank you. Is that just another way of saying -- is the conclusion or observation made in this document, the World Health Organization report, just another way of saying that the evidence -- the then available scientific evidence was insufficient or inadequate to conclude that 2,4-D was a carcinogen one way or the other?

A. I would like to change that a little bit, if I can. When one tests the null hypothesis; that is, it's very difficult in science to test a negative, so one can only test for the absence of a positive effect; that is, the question one asks of a cancer bioassay is: Did it cause cancer, rather than the converse: Did it not.

Now, what this sentence says is that the available information is inadequate to arrive at a conclusion, rather than saying that the available evidence did not suggest that it was a carcinogen. Those are different conclusions.

Q. All right. I tried not -- All right. I don't want to get into a semantical debate with you. My point is, they couldn't tell based on the evidence they had?

A. I'd agree with that.

Q. One way or the other?

A. That's right.

Q. All right. Which is, as you point out, different what the Ministry of the Environment expert panel said because they concluded in an affirmative sense that the evidence was insufficient to establish that it was carcinogenic?

A. That's correct. That's a very different version.

Q. Exactly. Thank you. Then if we could deal with the evaluation of health risks from 2,4-D exposure, section 9.4 page -- same page, am I correct that the report indicates that from the data available at present, the task group assumes that a possible health risk will exist when the safety factor is less than 100.

So just dealing with that, that is what the authors of this report concluded as a possible health risk standard or a benchmark, if I could put it that way?

A. That's right.

Q. All right. Their recommendations on exposure follow in the immediately following paragraph and to paraphrase it, could I suggest that they

indicate that the risks of exposure can be reduced fairly simply - that is their language - fairly simply by measures of occupational hygience, by proper laundering procedures, et cetera?

A. Yes.

Q. All right. So that whatever the risk might be, properly followed hygienic and protection procedures can minimize that risk?

A. Yes.

Q. All right. And, indeed, that is something that the -- well, in fairness to you I'll take you to the section.

Could I also ask you to go back to Chapter 1 which is a summary section of the document and which deals with effects on human beings of exposure to 2,4-D.

And I'm interested particularly in the section at page 12, Dr. Ritter, dealing with the uptake and fate of 2,4-D in the body and that is Section 1.1.5. Am I correct that the authors indicate that 2,4-D and its derivatives can be absorbed via the oral, dermal and inhalation routes? Stopping there.

A. Yes.

Q. And that general population exposure is mainly by the oral route but that under occupational

and bystander exposure conditions, the dermal route is by far the most important?

A. Yes, that's --

Q. I'm sorry.

A. That's generally been true in most investigations world-wide.

Q. All right. And that's just confirmation of the passage that we looked at a few moments ago?

A. Yes.

Q. All right. And then the point which I'm particularly interested in at the moment, am I correct that the authors indicate that there is no evidence that 2,4-D is accumulated throughout the body?

A. That's correct.

Q. All right.

A. In fact I would say that, if I may, that conclusion I would say has been strengthened considerably since the time that this work was done.

Q. All right. And indeed, just dealing with the state of information then available, recognizing as you have said that it has been strengthened today by the state of scientific knowledge, but even then the authors indicated that:

"Transformation of 2,4-D in mammals

appears to occur only to a slight extent and mainly involves the production of 2,4-D conjugates with sugars or amino acids and that a single dose was excreted within a few days."

A. That's what it says, yes.

Q. All right, thank you.

THE CHAIRMAN: Ms. Cronk, in view of the number of passages you've read, I think it would be advisable--

MS. CRONK: Fine, sir, I'll do it.

THE CHAIRMAN: --if we had copies of that put into evidence.

MS. CRONK: I apologize for not having it done. I read the report late last evening, but I'll get them for you.

THE CHAIRMAN: Thank you.

MS. CRONK: Q. Can we agree then, Dr. Ritter, and I would just like you to step back and consider the four documents that we've talked about over the last two days; first of all, the MOE expert panel document, the Bond article dealing with phenoxy herbicides generall, the Crump report, now the World Health Organization Report that we've referred to.

Just comparing the World Health report,

for example, and the MOE expert panel report, would you agree with me that the expert panel report prepared on behalf of and delivered to the Minister of the Environment in Ontario is clearly the more recent document and contains strongly worded conclusions regarding the possible carcinogenic effect of 2,4-D?

DR. RITTER: A. Yes.

Q. And, similarly, the World Health Organization report predates the Crump report and does not contain the same type of risk analysis data that is contained in the Crump report?

A. That's correct.

Q. The purposes were different?

A. Yes.

Q. All right. And, again, the conclusions expressed in the Crump report suggest negligible or low risks of cancer or non-carcinogenic health effects from exposure to 2,4-D and glyphosate under the conditions they examined?

A. Yes, that report says that.

Q. All right. Are you aware of any similar World Health Organization report regarding glyphosate?

A. No, none has been done to the best of my knowledge.

Q. All right. With respect to any of the pesticides in use in Ontario, bearing in mind the tables that we looked at yesterday, are you aware of any similar report by the World Health Organization or a like international body regarding any of those pesticides apart from 2,4-D?

A. There have been registration standards documents issued of various pesticides by the United States Environmental Protection Agency over the years, they are not an international body.

Q. I meant something like the World Health Organization?

A. No.

Q. A similar in-depth look at the scientific literature?

A. Not that I'm aware of.

Q. All right. Dr. Ritter, as we sit here today in August, 1989, given the published views of the scientific community on the public health risks of 2,4-D and glyphosate specifically, can you indicate to the Board whether it is your opinion that those products can be used safely for forestry uses, if used in accordance with the procedures authorized by the federal registration system and label instructions?

A. There are a number of studies which

are ongoing particularly with reference to 2,4-D, both in Canada and elsewhere. At the moment, certainly our view that 2,4-D and glyphosate when used in accordance with those label directions, should not constitute an unacceptable degree of hazard, particularly in the forestry sector.

Q. They are safe in the forestry sector when used in accordance with authorized procedures?

A. Safe is a term which we tend not to think of; we tend to think of varying degrees of risk, and I prefer my answer to yours.

Q. Well --

A. We would not consider that that use constitutes an unacceptable risk.

Q. All right. And that -- I understand that answer from a representative of a federal regulatory agency and I accept it as such, but what I'm asking you, however, as a layperson in the context of these hearings is:

Are members of the public entitled, in your view and based on your knowledge in this area, to assume, in light of the registration of those pesticides, that those products are safe for use in the forestry sector if used in accordance with properly authorized procedures?

A. In the context in which you've asked the question, I think the risks associated with the use of these products would fall within a range that most people would consider to be safe, yes.

Q. The answer was yes, Dr. Ritter?

A. Yes.

Q. Thank you. Is that also true of the other pesticides authorized for use in the forestry sector in Ontario based on your knowledge of them?

A. Based on my knowledge at this moment, yes.

Q. All right. I will put it to you quite simply: If there was any reliable or valid information to the contrary, your agency would be looking very hard at the question of -- or would in fact have removed that registration; would they not?

A. That's correct.

Q. All right. These products are still authorized for use out there and there is a reason for that?

A. That's right.

Q. Thank you. And, finally, the second report that you mentioned yesterday dealt with the farmer motality study that I understand is underway?

A. That's right.

Q. And did I understand you correctly, Dr. Ritter -- sorry, do I understand what you said correctly that that is a study being conducted by Health and Welfare?

A. That's correct.

Q. All right. And that -- just to recap what you said about it yesterday, the only results to date available are interim results emanating from the investigations done in the Province of Saskatchewan?

A. That's correct.

Q. Thank you.

THE CHAIRMAN: Dr. Ritter, has there been in your experience any situations where your agency has approved for registration a product, felt obviously at the time that the risk was reasonably low and, therefore, could be used with no appreciable health risk and then subsequently had to reverse your position?

DR. RITTER: Yes.

THE CHAIRMAN: And I take it the reasons for that were just additional studies which in effect proved that your earlier assessment was wrong?

DR. RITTER: That's correct.

THE CHAIRMAN: And is that, to your knowledge, the case with some of the spray programs

that I believe were conducted in eastern Canada?

DR. RITTER: No.

THE CHAIRMAN: There is one in Nova Scotia I believe that recently was -- I think there was a court case involving a particular spray program in Nova Scotia whereby they obtained a court injunction in terms of the use of that spray. Are you aware of that?

DR. RITTER: There have been a number of legal actions particularly in the Maritimes in the last 10 or 12 years. I'm not specifically familiar with the case that you cite, but...

THE CHAIRMAN: Okay, but those incidents wouldn't be ones that would normally come to the attention of your department?

DR. RITTER: They might. For example, representatives of my unit were deeply involved in what's become known as the 2,4-D trial, the injunction hearing before the Nova Scotia Supreme Court some years ago. We've been involved in other judicial proceedings on a variety of matters.

THE CHAIRMAN: But it has never resulted in your organization advocating the withdrawal of a registration of 2,4-D?

DR. RITTER: No.

THE CHAIRMAN: Okay.

MR. MARTEL: Is there one not just going on in the east coast within the last month in, I believe, New Brunswick?

DR. RITTER: On 2,4-D?

MR. MARTEL: Yes.

DR. RITTER: Yes. I believe Mr. Jerry White brought a motion in court seeking a permanent injunction.

MS. MURPHY: In fairness to the witness perhaps, and this is not necessarily something he would normally know a lot of details about, and I am just a little concerned that he is being asked to recount some information that --

THE CHAIRMAN: Well, we are not interested particularly, I don't think, in the court action per se. I think the information we are trying to obtain is whether his department, as a result of these court actions, have concerns with 2,4-D -- with the use of 2,4-D and the applications for which these court actions concern.

DR. RITTER: We certainly have an interest in the latter part of your question, and where health effects become a central issue in a court challenge we have always been involved because clearly that's our responsibility. But many of these court

challenges that have taken place over the years, as you can imagine, go outside of the realm of health effects. They may deal with trespass, all kinds of law.

THE CHAIRMAN: No, I think we are just interested really in the health effects and whether your department in its involvement, if any, with any of these court actions have had second thoughts or concerns relating to the continued registration of this product.

DR. RITTER: Never as a result of a court action. We have certainly moved to take products off of the market as a result of additional studies which may become available subsequent to the initial registration.

The most notable one we have referred to during the course of these proceedings Alachlor, but never as a result of a court action.

THE CHAIRMAN: Okay.

MS. CRONK: Mr. Chairman, the Nova Scotia case to which you refer has come up a number of times in the hearing. It is the Palmer decision, a decision of Mr. Justice Nunn. It is our position that the Board is entitled to take judicial notice of any reported case anywhere in the jurisdiction of Canada and we would propose to make a copy and provide them to the

Board so that you have it.

THE CHAIRMAN: Very well. Thank you, Ms. Cronk. We were just basically trying to tie in the results of those sort of notorious cases with any action that your department might have taken or been concerned with concerning the continued registration of the products that are before this Board.

DR. RITTER: We took no action as a result of that decision by Mr. Nunn in Nova Scotia.

MS. CRONK: Q. And I take it it follows from that, Dr. Ritter, that notwithstanding any other litigation in this country of which your department is aware dealing with public health effects of 2,4-D, that has not caused your department to seek, to rescind, remove or tract or narrow the current registration authorizations for the use of 2,4-D?

DR. RITTER: A. That's correct.

Q. That is also true with respect to glyphosate; is it not?

A. Yes, it is.

Q. And that is also true with respect to the other pesticides currently authorized for use in forestry in Ontario?

A. That's correct.

Q. Thank you.

THE CHAIRMAN: Thank you.

MS. CRONK: My colleague Mr. Cassidy has just reminded me - and I know, Mr. Chairman, that the Board is aware of this - that the injunction request was denied in the Palmer case, if that needed -- was of assistance to have that drawn to your attention again.

THE CHAIRMAN: Thank you.

MS. CRONK: Q. Dr. Ritter, then, referring to the farmer mortality - referring - returning to the farmer mortality study that we were discussing a moment ago, I am going to show you a photocopy of an abstract form which was provided to me by you yesterday and I would ask you whether this is the information available on the public record in official form concerning the results to date of that study?

DR. RITTER: A. That is the document that I provided to you yesterday including the handwritten notation which I have added to the bottom.

Q. That is your handwriting at the bottom?

A. That's my handwriting.

MS. CRONK: (handed)

THE CHAIRMAN: That will be Exhibit 717.

Thank you.

---EXHIBIT NO. 717: Photocopy of abstract re: farmer mortality study provided by Dr. Ritter.

MS. CRONK: Q. Dr. Ritter, before we proceed to discuss the contents of this abstract, as the photocopying of your handwritten notation was faint, I wonder if you could simply read into the record what your handwritten addition to the bottom of the abstract is?

DR. RITTER: A. Yes. It says:

"A similar risk relationship was established between expenditure on fuel and risk of NHL independent of total acres sprayed."

Q. Could I have it again, please?

A. "A similar risk relationship was established between expenditure on fuel and risk of NHL independent of total acres sprayed."

Q. I will come back to that in a moment. Am I correct, Dr. Ritter, that the first paragraph of this abstract relates or sets out the background to the study?

A. That's correct.

Q. And it indicates, and I quote:

"Public concern over the health hazards

of pesticides remains high with a major focus on the risk of cancer in a significant but lower level of concern about adverse effects on pregnancy and children."

Then the last sentence reads:

"This concern persists despite the fact that there is little evidence of physical health effects due to exposure of the public to pesticides and other environmental pollutants."

Stopping there for a moment, Dr. Ritter. Who was the author of this abstract?

A. There were five of us. These are the joint collaborators on this study which are drawn from the Centre for Disease Control, Laboratory Centre for Disease Control of Department of Health and Welfare and the Environmental Health Directorate, my group of the Department of Health and Welfare.

Q. All right. Were you then either one of the five authors of this abstract or did you supervise those persons from your department who were involved in its production?

A. Both.

Q. All right. Do you agree with the

last sentence that I just read to you?

A. Yes, I do.

Q. All right. And then looking at paragraph 2, as I understand it, the first sentence of that paragraph sets out the reason for the study, that being to assess the risks of cancer in relation to pesticide exposure indices among the largest population sub-group with direct, long-term occupational exposure; that is to say, to assess the risk of cancer among farmers?

A. That's correct.

Q. All right. And that for that purpose the methodology adopted was to examine mortality among 365,000 farm operators identified in the 1971 census of agriculture?

A. Yes.

Q. And having done that, to link it to the national mortality database for the years 1971 to 1988 inclusive; that is to say, for a 16-year period?

A. Yes.

Q. And then the next sentence, as I understand it, sets out the results in the Province of Saskatchewan based on the study that has been carried out, and am I correct that the results for the Saskatchewan component indicate that farmers had lower

risks of death for all conditions combined and for several major disease categories including all cancers and including none-Hodgkins lymphoma than was expected?

A. That's correct.

Q. All right. Do I take from that then that these results - and the numbers are actually here, I have just read the text of the conclusion - that this study in the Province of Saskatchewan demonstrates a lower risk of mortality than would have been expected in the grouping of farmers exposed to pesticides?

A. Yes.

Q. All right. And from the perspective then of public health, human health risk, I take it these test results are very encouraging; you'd agree?

A. Yes.

Q. And then, finally, in the next sentence -- I'm sorry, and could I go back and add one other thing. To the extent then -- to the extent then that any press reports suggested contrary findings in the Province of Saskatchewan as a result of this study, those reports were inaccurate?

A. That's correct.

Q. These results in fact show the lowering of risks of death for all conditions combined and for all cancers?

A. That's correct.

Q. All right. You then go on to indicate in the next sentence:

"However, on farms less than 1,000 acres there was a statistically significant exposure risk relation between risk of non-Hodgkin's lymphoma and acreage sprayed for weeds as reported in the 1971 census of agriculture."

Now stopping there. What does that mean? In layman's terms, Dr. Ritter, what's being said there?

A. There are two things that are being said in this paragraph. The first is that the acronym in parentheses, SMR, is the standardized mortality ratio which is the expression of the observed number of deaths when compared to the expected number of deaths over that period of time.

So that an SMR of less than one is an expression of a desirable effect, if you like. An SMR of less than one implies that fewer people died than you would have predicted from national mortality figures.

These SMRs that you see for deaths, for all forms of cancer and for NHL specifically are all less than one.

Q. NHL being non-Hodgkin's lymphoma?

A. That's correct. So for these three variables in all three cases the actual number of deaths observed was considerably below what one might have expected based on averages for the Province of Saskatchewan.

Q. And from a statistical perspective that is an affirmative, a positive, an encouraging result?

A. That's correct. To put it into perspective, as I recall one might have expected about 16,000 deaths.

There were 70,000 farmers that were examined in the Saskatchewan cohort. Of the 70,000 farmers for that period of investigation, from 1971 to 1988, one might have expected approximately 16,000 deaths in that cohort of 70,000 people. Of that expectation of 16,000 we noted only approximately 12,000 deaths.

Q. All right, i.e., less than was expected?

A. About one quarter less.

Q. All right.

THE CHAIRMAN: Is that from all causes?

DR. RITTER: That's death.

THE CHAIRMAN: Death.

MS. CRONK: Q. Well, death from what is the Chairman's question.

DR. RITTER: A. Well, no, because we then go on to talk about death from all forms of cancer. But the first thing one does in this kind of a study, because death is relatively easy to interpret in terms of this kind of an investigation, we look at the absolute possibility and that's just death.

Q. What was the standardized mortality ratio for smaller farms, or do you remember?

A. I don't remember. The point really that I was going to make was that the expected incidence of non-Hodgkin's lymphoma -- the actual incidence of non-Hodgkin's lymphoma in the Province of Saskatchewan was somewhat less than the expected incidence of non-Hodgkin's lymphoma.

I would not attach a great deal of importance to the fact that it's slightly below one. I would, for all practical purposes, say that the incidence of non-Hodgkin's lymphoma among Saskatchewan farmers was more or less comparable to what one might have expected. It was no greater than what one might have expected.

We found that observation noteworthy

because there are more phenoxy herbicides used in the Province of Saskatchewan. I believe our latest estimates suggest that there are more used in Saskatchewan than in the rest of Canada combined.

Q. I wanted to ask you about that. That, given the number of farmers in the province and the extent of farming operations in that province; that is, Saskatchewan, I take it you are saying that overall there are more pesticides of that kind used in that province than anywhere else in Canada?

A. I believe more are used in that province than the rest of Canada combined.

Q. All right. That being the case and these results being a reflection of the mortality statistics for Saskatchewan, would you agree with me that the results when available for other provinces in Canada necessarily will be lower, given that the usage is lower?

A. They will be --

Q. You would expect them to be?

A. They will be very difficult to explain if they are not.

Q. All right. It would be your expectation--

A. Yes.

Q. --that they would have to be lower?

A. Yes.

Q. All right.

A. Going on, once we've established that the risk of non-Hodgkin's lymphoma in the Province of Saskatchewan was actually comparable among farmers as it was among non-farmers, we then went on to look at what was actually driving that risk, however low it may have been, and we made a couple of observations with regards to what may have been driving that risk in Saskatchewan.

Among other things, we noted that there appeared to be a positive association between the number of acres sprayed for weeds and the risk of that non-Hodgkin's lymphoma even though the risk was no greater than it was for people who don't farm at all.

Now, I emphasize that the relationship is to a fuction we call spraying for weeds and it is not a study of the relationship between the exposure to a chemical and the risk of non-Hodgkin's lymphoma.

What I am tying to say is that this is not a study of 2,4-D or glyphosate or anything else specifically, it is a study of a practice, a function on occupational activity as a fuction of risk and, in this particular case, the function is spraying for

weeds.

But there are many variables in the function that we call spraying for weeds, one of which most certainly includes exposure to herbicides, but it's only one of several variables. For example, one would expect that there would be concomitant exposure to a wide variety of solvents in that function we call spraying for weeds, and so on and so forth.

It would, in my view, be premature and incorrect to read that sentence to mean anything other than what I just said. It is not a study of the relationship between any specific chemical and any specific disease.

We also found that there was a similar positive risk relationship but not defined by any acreage when one compared the dollars expended on fuel oil which presumably we use as a surrogate for exposure to fuel and the risk of non-Hodgkin's lymphoma.

Now, we felt what made that second observation even more interesting than the first was that in the first case, when we looked at acres sprayed for weeds, the risk relationship fell apart beyond a thousand acres and we don't have a ready explanation for that.

In the first instance it's tempting to

speculate that that may be the case because on farms larger than a thousand acres one might imagine that the spraying operation would be contracted out and, consequently, the individual farmer would no longer be taking the risk and so the risk relationship might fall apart on farms larger than a thousand acres. On reflection we find that that may not be correct, it may actually be the converse.

The information which we are gathering suggests, at least in its preliminary phase, that it may well be the smaller farms in which the spray operation is contracted out because farmers with only a few hundred acres may not be prepared to make the investment in the spray rig necessary to spray their own fields, and the larger farms may be spraying their own fields.

So if that's true, I would have no biologically plausible explanation for that observation in spite of the fact that the observation stands.

Q. All right. Can I just see if I understand all of this, Dr. Ritter, with respect to this issue or at least what is important that be understood from it. The first, as I understand it, what you have said is that the relationship was found to exist?

A. Yes.

Q. All right. It was found to exist with respect to farms no larger than a thousand areas?

A. That's correct.

Q. All right. But do I also understand you to have said, given the number of exposures or components involved in the spraying of weeds, that one should not conclude and it would be inaccurate to conclude that the relationship exists because of exposure to pesticides?

A. That's correct.

Q. Thank you. Can we leave it there, or is there something more important that I should know about that?

A. No, you can leave it there.

THE CHAIRMAN: Dr. Ritter, the other statement you made about the expenditure of dollars on fuel, are you making the assumption that the fuel exposure is what, because the fuel would be used in combines and tractors--

DR. RITTER: That's correct.

THE CHAIRMAN: --and there would be long -- or exposure over long periods of time?

DR. RITTER: That's correct. The other reason we were interested in the fuel relationship

specifically, now that you have asked the question, is because there is experimental evidence in the literature which suggests a possible association between lymphoid tumors and exposure to these diesel fuel components.

Lymphoid tumors were the ones which had been implicated in some of the 2,4-D data in the last five to ten years. So that there is substantial biological plausibility, if you like, to the observation of a risk relationship between what we're measuring, which is expenditure which we are using as a surrogate for exposure, and the risk of non-Hodgkin's lymphoma. What strengthens that observation, in our view, is the fact that it's independent of the number of acres. So the more you buy the higher your risk, plain and simple.

The earlier observation with regards to the number of acres sprayed is less clear because the relationship does fall apart at a specified acreage, and unless and until we can come up with a rational and biological explanation for why that should be so, it is difficult in our view to attach a great deal of importance to the observation in itself.

THE CHAIRMAN: Thank you.

MR. MARTEL: Were you able to track down

or did you attempt to trace the number of farmers who sold their property and moved to, let's say, Victoria or B.C. in attempting to get all of the deaths that occurred, or you simply relied on Saskatchewan statistics?

DR. RITTER: No, actually we didn't rely on Saskatchewan statistics at all. The registry is based -- the national mortality database, as the name implies, is based on national mortality statistics. So that if you die in Canada, regardless of where you farmed, you show up in the national mortality database.

In addition, because this is a linkage study, that vital statistic was then cross-linked to your place of residence and occupation at the time that you were living.

I might also add that we have, through a cooperative arrangement, exchange information between a number of U.S. states which are known to be popular for Canadians to retire and they include, I believe, the State of Florida, the State of Arizona, the State of California, the State of New York, and I think there are three others.

So that even if as a Canadian you were to die in one of those states, that death statistic would show up in our national mortality database.

MR. MARTEL: Thank you.

THE CHAIRMAN: Thank you. Ms. Cronk, are you going to be much longer? How long do you think you will be?

MS. CRONK: I will be I anticipate about another ten minutes with Dr. Ritter and then I have some questions for Mr. Kingsbury. Would you prefer to rise now?

THE CHAIRMAN: I think we will take the morning break at this time.

MS. CRONK: That's fine. Thank you, sir.

THE CHAIRMAN: 20 minutes. Thank you.

---Recess taken at 10:00 a.m.

---On resuming at 10:30 a.m.

THE CHAIRMAN: Thank you. Be seated, please.

MS. CRONK: Thank you, Mr. Chairman. Could we reserve a number, sir, for the extracts from the World Health Organization report that I will have photocopied and provide next week.

THE CHAIRMAN: Exhibit 718.

MS. CRONK: Thank you.

---EXHIBIT NO. 718: Extracts from World Health Organization Report. (Reserved)

MS. CRONK: Q. It also occurs to me,

really only as a suggestion to Ms. Murphy. That there were some extracts from one of Mr. Kingsbury's paper yesterday that were not -- the overheads were not identical to those contained in the paper, and I recall that she said she was going to have photocopies made and perhaps she would like a number reserved.

MS. MURPHY: That would be wise. There are a couple that are slightly different than the slides, so it would just be photocopies of two or three slides. One exhibit number would probably be sufficient.

THE CHAIRMAN: Okay. Exhibit 719 for that.

---EXHIBIT NO. 719: (Reserved) Extracts from paper by Mr. Kingsbury.

MS. CRONK: Thank you.

Q. Dr. Ritter, you will recall yesterday during our discussion of the MOE expert panel report on the carcinogenicity of 2,4-D I asked you whether the report had been accepted by Ontario's Minister of the Environment, Mr. Bradley, and your indication to the Board was, yes, that was your understanding.

I have now obtained a copy of a news release dated April 16, 1987 released by the Ontario Ministry of the Environment with respect to that

report.

MS. CRONK: I would like that, sir, to be the next exhibit.

THE CHAIRMAN: Exhibit 720.

---EXHIBIT NO. 720: News Release from Ontario Ministry of the Environment dated April 16, 1987.

THE CHAIRMAN: Dr. Ritter, while these are being handed out, in this expert panel report, Exhibit 714, there is a reference at the back under the bibliography to the - just a moment - to the World Health Organization 1984. It's on page 61. Is that the report--

DR. RITTER: Yes, it is.

THE CHAIRMAN: --that you just dealt with?

DR. RITTER: Yes.

THE CHAIRMAN: Thank you.

MS. CRONK: Q. Dr. Ritter, with respect to this press release, as I understand it, if I could attempt to summarize it and I would ask for your confirmation of it, with the emergence in 1986 in the United States of a number of studies, two in fact, suggesting that 2,4-D might be a carcinogen, the Ontario Minister of the Environment placed a moratorium on the use in Ontario of new products containing 2,4-D

until a review of those studies could be conducted; am I correct in that?

DR. RITTER: A. That's correct.

Q. And it was for the purposes of reviewing that data and the existing state of the scientific literature that the Minister appointed the panel of experts who ultimately produced the report that has been marked before this Board; is that correct?

A. I believe that's correct, yes.

Q. All right. And this press release on page 2 confirms; does it not, that that panel of experts appointed by the Minister reviewed all current studies including the two at issue from the United States, being a report of the U.S. National Cancer Institute and a report -- an epidemiological study from the State of Kansas?

A. Those are the same studies.

Q. All right.

A. The study done in Kansas was conducted by the U.S. National Cancer Institute.

Q. Sorry. And the second study then that had sparked the issue, if I could put it that way, in 1986 was a study completed by an industry task force and that report as well was reviewed by the panel of

experts appointed by the Minister of the Environment?

A. That's correct.

Q. All right. And am I also correct that in this press release the Environment Minister, Jim Bradley, announced that new products containing the herbicide 2,4-D would be allowed in Ontario as and from that date?

A. Yes.

Q. And the press release goes on to indicate that this decision followed from a recommendation made to the Minister by the Ontario Pesticide Advisory Committee; is that correct?

A. Yes.

Q. And that committee had reviewed the expert panel report on 2,4-D carcinogenicity which has been marked as an exhibit before this Board?

A. Yes.

Q. And the press release indicates that a Dr. John Doole of the Department of Pharmacologyy, Toxicology and Therapeutics at the University of Kansas reviewed the expert panel report on behalf of the Ontario Pesticide Advisory Committee. Am I right?

A. Yes.

Q. And that based on the conclusions of the expert panel, the advisory committee advised the

Minister:

"No changes be made to current provincial regulatory requirements governing the use of 2,4-D at this time and, further, OPAC also advised the Minister to lift the ban on new 2,4-D products."

A. That's correct.

Q. Those were the recommendations of the committee?

A. Yes.

Q. And they followed upon a review and consideration of the expert panel report?

A. Yes.

Q. All right. Am I correct then that this press release confirms that the Minister of the Environment of the day accepted the report, received the recommendations of his advisory committee in light of the report and acted upon them accordingly?

A. Yes.

Q. Thank you. There is one further report, Dr. Ritter, that I would ask for your comment and assistance on. It deals with the issue of insecticides.

It is an article published in January of 1984 in a publication entitled: Applied and

Environmental Microbiology. It is entitled: Assessment of the Potential of Insecticides, Emulsifiers and Solvent Mixtures to Enhance Viral Infection in Cultured Mammalian cells.

And I understand that you were one of the study coordinators for that study that resulted in the ultimate publication of the article. Is that correct?

A. That's correct.

MS. CRONK: (handed)

THE CHAIRMAN: Exhibit 721.

---EXHIBIT NO. 721: Article entitled: Assessment of the Potential of Insecticides, Emulsifiers and Solvent Mixtures to Enhance Viral Infection in Cultured Mammalian Cells published in Applied and Environmental Microbiology.

MS. CRONK: As I understand it, Dr. Ritter, there were two study coordinators for the purpose of this study. You were one, the other was Dr. D. G. Ecobichon; is that correct?

DR. RITTER: A. That's correct.

Q. Do you see Dr. Ecobichon in the room today?

A. He's one over from you.

Q. Thank you. And Dr. Ecobichon; am I correct, is attached to the Department of Pharmacology,

Toxicology and Therapeutics at McGill University in Montreal?

A. That's correct.

Q. All right. Can you explain to the Board, please, what the purpose of this study was?

A. In the early 1980s, latter 1970s, there had been a number of reports originating at Dalhousie University in Halifax which suggested that there may be an association between the use of insecticides in the Maritime province -- in the Maritime Provinces in the forestry spray programs and the incidence of Reye's Syndrome which is a pediatric disorder frequently resulting in death in those provinces.

We became interested in it. Dr. Ecobichon and I became interested in that possibility and thought that it might be useful if we could be, in the first instance, attempt to replicate some of the results which had been -- some of the experiments which had been published by these investigators from Dalhousie University and, in the second place, to perhaps advance that technology somewhat.

We undertook that study, therefore, and to try to ensure the most reliable results we had the study protocol varified by the initial study author,

that was Dr. Kenneth Rozee who at the time was with the Department of Microbiology at Dalhousie.

Upon verification of our protocol by Dr. Rozee, we then approached three independent laboratories which included Bio Research Laboratories in Montreal, the largest contract research facility in Canada.

Dr. C. Y. Chang, who was head of the Division of Microbiology and Immunology at the Ottawa General Hospital and Dr. J. Thorson who was with the Division of, I think, Infectious Disease at the Ontario Veterinary College.

Q. Why three laboratories?

A. Simply to increase the precision, the reliability of the result. I mean, one could have had it done in one, but we operated on the assumption that if we did it in three the results would be more reliable than in one.

Q. At whose behalf or whose request was the study carried out?

A. The study was really done at our initiation but was funded by Forest Protection Limited in New Brunswick.

Q. All right. And did the Canadian Forestry Service have any involvement or interest in

the work?

A. These are events that took place now almost seven years ago. They had interest in it, I don't know that I would say they had involvement in it.

Q. All right. Perhaps, in fairness to you, could I refer you to the first paragraph of the article, approximately 10 -- 15 lines down in which it indicates:

"In an attempt to replicate results previously published..."

Stopping there. I take it that the previously published results were those which were used in an effort to associate insecticide spraying in New Brunswick with an incidence of Reye's Syndrome?

A. That's correct.

Q. All right.

A. And if you were to check citation No. 5 it refers to K. R. Rozee the investigator that I referred to few moments ago.

Q. So the reports were his?

A. That's right.

Q. All right.

THE CHAIRMAN: Dr. Ritter, is this the same disease associated with -- sometimes with teenagers taking aspirin?

DR. RITTER: Yes, it is. It was subsequent to this work, I might even be so bold to suggest that in part because of this work, that the U.S. Centre for Disease Control and the U.S. National Institutes of Health undertook some rather ambitious studies to took at the incidence and possible etiology of Reye's Syndrome in the United States and after a rather extended period of investigation, came to the conclusion that the primary factors involved in the etiology of Reye's Syndrome were aspirin following a particular illness.

And subsequent to that, as you know, both the American and Canadian Pediatric Academies have recommended that children at risk do not take aspirin with these diseases.

THE CHAIRMAN: Thank you.

MS. CRONK: Q. Continuing on in the sentence to which I have directed your attention, it indicates; does it not, that the Canadian Forestry Service had suppressed an interest in having the experiment outlined in the previously published literature validated?

DR. RITTER: A. That's correct.

Q. All right. And that, in part I take it, led to your interest and that of Dr. Ecobichon in

carrying out this study and in devising the protocol for the study?

A. That's correct.

Q. All right. And then you were explaining to the Board that you approached and arranged for three independent laboratories to carry out the appropriate tests?

A. That's right.

Q. Am I correct that of the components tested, they included three chemical insecticides Madacil, 7, and Fenitrothion?

A. That's right.

Q. All right. And could you outline as reflected in the discussion section of the paper, beginning over at page 83, could you outline to the Board the results of the study that you conducted with Dr. Ecobichon?

A. Essentially we found two critical -- we made two critical observations from the study. I should perhaps just add, in terms of the protocol, again in our attempt to make the results as reliable as possible, the study was done in a double blind fashion; that is, neither Dr. Ecobichon nor I nor the investigators had any knowledge of the chemicals that they were testing. They were individually coded and

the codes for the three participating laboratories were different.

So that Code A, for example, for Lab 1 contained a different chemical than Code A for Lab 2 so that there could not be any collaboration between laboratories or between the study coordinators; that is, Dr. Ecobichon and myself, and the laboratories. That code was only disclosed after the results were in.

Q. All right.

A. The results -- the experiments essentially provided us with two sets of observations. The first and perhaps foremost was that we were ununable to replicate the results which Dr. Rozee had published previously based on a protocol in which he had participated in its development and had subsequently approved.

Q. All right. Stopping there for a moment. In the scientific community, as part of a scientific investigation, what is the significance of replication or lack there of results?

A. It's absolutely essential. In the absence of an ability to replicate results, I think one would have to wonder very seriously about the validity of the result. It's the hallmark of scientific investigation.

Q. And the second observation?

A. The second observation we made is that none of these agents essentially had the capacity to enhance viral replication and, what I mean by that is, this protocol was designed to study the capacity of these agents, the ability of these agents to allow viruses to replicate at rates that would be considered to be unusual; that is, to enhance viral infection, that was the intent of the study.

The first observation, as I said, was that we were unable to replicate the result. The second observation was notwithstanding the first none of the agents tests, and there was I believe some combination of 40 odd agents that were tested in this study, none of the chemicals tested had the capacity to increase the potential for viral infection.

Q. Including the three chemical insecticides to which I drew your attention a moment ago?

A. Including the three chemicals.

Q. All right. Bearing in mind then the two observations that flowed from this work, what is your opinion regarding the suggestion reported in the scientific literature in the early 1980s regarding a possible association between insecticide spraying and

Reye's Syndrome?

A. In our view there is no association.

Q. Are you aware of any subsequent scientific work or investigation which causes you to reconsider that opinion in any way?

A. No.

Q. Are you aware of any such work which cast doubt upon the results obtained in this study?

A. No.

Q. What is the area of Dr. Ecobichon's expertise?

A. Pharmacology, toxicology.

Q. Is he an acknowledged expert, in your view, in those fields?

A. Yes, he is.

Q. With particular expertise in insecticides?

A. Yes.

Q. Thank you.

THE CHAIRMAN: Dr. Ritter, how would a suspicion arise in the first place if it wasn't based on some scientific study?

When you refer broadly to: There was some indication in the scientific literature that there was a correlation between Reye's Syndrome and some of

these agents, how would that conclusion be reached so that it would reach scientific -- the status of being considered part of the scientific literature in the first place if there weren't studies that would tend to show perhaps opposite results to what you found?

DR. RITTER: The impetus for the work came originally in the latter 1970s when Dr. John Crocker, who is a pediatric nephrologist at the Children's Hospital in Halifax suggested that he felt that there were an unusually large number of cases of Reye's Syndrome in the Maritime Provinces.

The Children's Facility in Halifax being the regional referral centre for pediatric medicine would have put Dr. Crocker in the unique position of having examined virtually all of the children with the disease in the Maritime Provinces.

They felt, at least on their preliminary observation, that there appeared to be a higher incidence than they may have expected. That led them to a series of experimental trials on which they based their original hypothesis that there was an association between the two events.

In our view the initial observation from the cases admitted at the hospital as well as the subsequent experimental trials were flawed both in

their logic and in their conduct and, consequently, we were prompted to what we felt was to undertake a somewhat more exhaustive and more credibly scientific approach to that investigation.

For example, in the case of the human trials -- we felt that in the case of the human disease, we felt that the number of cases of Reye's Syndrome that might occur in the Maritime Provinces was probably too low, below the threshold, because the disease has a relatively -- is relatively uncommon, has a very low background rate and the population at risk in the Maritime Provinces we felt was sufficiently small and that it was unlikely that even if the disease were going on, it would necessarily be expressed. So we felt that this kind of thing really required a more intensive investigation.

I should add perhaps that, which has not become evident here, we were not the only ones that were asked to examine this issue. Dr. Walter Spitzer who was Chief of Medicine at McGill University at the time was also asked by the Government of New Brunswick to examine the possible association between Reye's Syndrome and the forestry spray programs in the Province of New Brunswick.

And to summarize just from memory,

essentially what Dr. Spitzer concluded at the time after his investigation was that he could find no evidence of any possible association between the two events.

THE CHAIRMAN: Would it be normal in scientific circles after you completed a study such as the one you coordinated to either correspond or consult with the authors of a previous study that came to an opposite conclusion? Would there be discussion of, say, your results with the results obtained by those preceding you which came to opposite conclusions? For instance, have you talked to this doctor in Halifax--

DR. RITTER: Yes.

THE CHAIRMAN: --since your study, in the light of your own results?

DR. RITTER: Yes. There were discussions which ensued between ourselves and Dr. Rozee, both during the conduct of the study as well as in the period immediately after the study, as well as during the time that Dr. Spitzer was investigating the issue as well.

THE CHAIRMAN: And what would your opinion be as to the state of the scientific opinion on this issue at this time as a result of the initial study by this doctor in Halifax plus the two subsequent

studies?

DR. RITTER: Well, I should perhaps remind you that we had approached Dr. Rozee and asked him to participate in the conduct of this study. We had in fact requested that he be laboratory No. 4.

He indicated at the time that he was busy and that the undertaking would be expensive. We offered at the time to underwrite any costs, including the provision of additional staff so that it would not create a burden on his workload to participate in the study.

We felt it would have been very important to have Dr. Rozee as one of the four collaborators in a double blind study of this type. He declined that offer.

I think it would be fair to say that following our investigation and that of Dr. Spitzer's, and not necessarily because of it, there has been very little in the literature, if anything, other than the odd isolated report from the initial laboratory of Dr. Rozee's in which there has been any further attempt to associate Reye's Syndrome and forestry insecticide spray programs.

THE CHAIRMAN: Thank you.

MRS. KOVEN: Was Dr. Spitzer's work

epidemiological or did it involve some lab tests and diagnostic work?

DR. RITTER: It involved considerable diagnostic work. Dr. Spitzer is a clinician and, at the risk of using a cliche, I'm glad you asked that question.

One of the initial observations that Dr. Spitzer made -- he had requested charts of patients and there was quite a bit of controversy at the time because he was initially denied access to those charts from the Children's Hospital. He felt very strongly that he needed to see those charts and felt that the denial to access those charts was really a violation of a trust because he too was a physician and that there was really no legitimate reason why he should be denied access to those charts.

He went on to conclude in his report to the Government of New Brunswick in answer perhaps, and further answer to the Chairman's question, that part of the reason why these investigators may have arrived at an erroneous conclusion is because the data which they used had been seriously flawed.

He found, for example, that of the substantial number of cases of Reye's Syndrome which they had examined at the Children Hospital in Halifax

were in fact not from the region that had been sprayed at all, in fact a number weren't even Canadian.

Now, if one makes the assumption that your cases are all exposed and one establishes a risk relationship on that basis, obviously you are going to arrive at a very different conclusion than if a substantial proportion of those cases had never been exposed at all. And, in fact, that is exactly what had happened in the case of the Rozee investigation. There were a significant number of his cases that had never been exposed to these forestry insecticide spray programs at all.

That was an omission which Dr. Spitzer felt was rather serious -- an error which he felt was rather serious and, among other things, had requested that there be a correction issued through the Canadian Medical Association Journal and although the correction ultimately was prepared by Dr. Rozee and submitted to the Journal, needless to say it's the original publication that stands and one rarely references the correction.

So that the original paper suggests an association between Reye's Syndrome in children and the forest industry spray program and the fact that that conclusion is probably entirely erroneous is not nearly

as evident as the original suggestion.

There were a number of other flaws in the logic, as I say, that developed during the course of Dr. Spitzer's investigation, but that was I think potentially perhaps the most serious.

MRS. KOVEN: The problem with establishing exposure isn't usually that straightforward.

DR. RITTER: Well, what it was really based on was -- their investigation was based on the assumption that these children had come from areas that had been regularly sprayed with forestry insecticides.

It became evident during the course of Dr. Spitzer's work that many of these children had never been near any area that had been sprayed with forestry insecticide.

So that I think Dr. Spitzer attempted to eliminate that element of doubt by looking at -- rather than looking at probably exposed categories, he looked at in all likelihood never exposed as compared to possibly exposed. So that I think he gave the benefit of the doubt to the investigation, if you like, but it was evident that there were a significant number of cases that it was virtually impossible to imagine how they ever would have been exposed to any of these

sprays.

There was another rather serious microbiological error in some of the assumptions which Dr. Rozee and his investigators had made as well, and I won't belabour you with what that was, unless you want to hear it. But, again, a number of people involved in infectious disease who had looked at that work, including Dr. Chang, felt that the hypothesis was sufficiently flawed in its foundation that the conclusion was hardly credible.

MRS. KOVEN: And, therefore, there is no perspective follow up on that data or on that population of children?

DR. RITTER: It would be very difficult to follow that population in any case because, as I indicated, given the background incidence of Reye's Syndrome and given the population at risk in the Maritimes, it is unlikely that one would be able to detect an increase unless it were virtually epidemic in a population of that size.

But the Centre for Disease Control in the United States in Atlanta did undertake an investigation on the etiology of Reye's Syndrome and their conclusion was that probably the single largest factor, if not the sole factor, that was driving the incidence of Reye's

Syndrome at least in the United States was the use of aspirin in children particularly who were affected either by influenza-B or varicella chicken pox.

And, as you know, there have been warnings that have been issued since that time that have suggested that -- well, depends on academy you follow. Some have suggested that aspirin not be used in children at risk age at all because you really don't know early on in the disease if they have chicken pox or influenza-B. Other sources have been somewhat less concerned and said that if there is a diagnosis of either influenza-B or chicken pox, aspirin should not be used as an analgesic in these children. They both mean essentially the same thing, one is a little more cautious than the other.

But, as I indicated, to the Chairman, I know of no work, credible or otherwise, that has been published since this time other than the odd isolated report which followed immediately after this work which has suggested an association between these kinds of agent and Reye's Syndrome.

MRS. KOVEN: Was Spitzer able to establish a profile of exposure in the paths that would identify specific changes in the use of sprays or the product composition? Often in that sort of study they

are able to say: Yes, there was a window of exposure that changed significantly over time as practices improved. So in fact, the concern would be very much a past exposure.

DR. RITTER: But Reye's Syndrome is not a disease of chronic onset and one would not have expected that a historical alteration in exposure potential would have altered the incidence of the disease; that is, an exposure practice which may have been in Vogue ten years ago, an exposure scenario that may have been popular 10 years ago is unlikely to affect the incidence of Reye's Syndrome today.

It is a disease which is caused by whatever the agent is, and let's assume it's aspirin, it's a disease which is caused by very, very contemporary exposure to the agent unlike cancer. So that the kind of suggestion you make would probably not be very useful in this kind of a disease.

MS. CRONK: Q. Dr. Ritter, at the risk briefly of inviting you to be modest, is it fair to say that the work done by Dr. Ecobichon and yourself, as reported in this paper, put to rest the suggestion in the scientific community of an association between Reye's Syndrome and use of these insecticides?

A. I think I would prefer to say that

the work that we coordinated, the work that we did suggested that the original experimental protocol was not credible. I don't know that we made a useful contribution to the scientific literature other than to establish the validity of an earlier protocol.

Q. Well perhaps others, apart from yourself, will be prepared to draw some inference from the fact that it hasn't been dealt with in the literature since.

There is one other aspect, though, of the work that Dr. Spitzer did in light of your conversation with Mrs. Koven that I would like to pursue.

It is my understanding that Dr. Spitzer had another set of experiments done similar to those which Dr. Ecobichon and yourself had arranged to have done in this report and that they were conducted at laboratories in the midwest United States. Are you familiar with those?

A. Yes, I am.

Q. And what were the results of that duplication of your study experiments, if you will?

A. They were similar to our own.

Q. All right. I would like the purpose for bringing this to the attention of the Board to be clear in my discussion with you, Dr. Ritter.

Would it be fair to suggest that the controversy initially occasioned by the published literature suggesting an association between Reye's Syndrome and the use of insecticides is an example of the kind of issue that can arise from time to time in the scientific community concerning the public health aspects of a variety of chemical agents?

A. Yes.

Q. It happens from time to time?

A. It happens with some regularity and frequency.

Q. I'm glad you said that. And is it also not the case -- well, when an issue of that kind arises, particularly with respect to the public health implications of chemical agents, is it fair to say that they become and receive critical public health attention because there's the possibility that they may be true?

A. Yes.

Q. All right. And that leads, does it not, on occasion to concerns in the minds of the public about the possible public health effects of the use of chemicals that, in many instances, are not from a scientific point of view valid?

A. That's correct. Our motive, as I

mentioned, for undertaking this work, for example, was not to discredit Dr. Rozee or Dr. Crocker; our motive was because they had presented a published hypothesis that suggested a possible association between a very serious and often fatal disease and the use of insecticides, and that was a possibility that I felt we had to deal with very seriously.

Q. Because, were it true, it would be a very serious public health concern; isn't that so?

A. That's correct.

Q. All right. And isn't the very issue that I'm putting to you now what your group was alluding to in the introduction to the abstract from the farmer mortality study when you indicate that a concern over the health hazards of pesticides remains high in the public, notwithstanding the fact that there is little evidence of physical health effects due to exposure of the public to pesticides and other environmental pollutants?

A. That's correct. We see the role of a public health agency to be -- to include investigation of any plausible hypothesis of an association between human disease and use practices, in my particular case, in association with use of pesticides, and because the possible risks associated with pesticide use are

plausible, these agents are, after all is said and done, toxic agents often intended to kill something, it's a plausible hypothesis.

And, consequently, we take those kinds of suggestions, such as the ones made by Drs. Rozee and Crocker and others that have made suggestions with regard to farmers and pesticides, we take those suggestions very seriously.

Q. And you would agree with me that that clearly is appropriate because they could represent real public health hazards--

A. Yes.

Q. --were they substantiated?

A. That's correct.

Q. Yes.

THE CHAIRMAN: Dr. Ritter, in fairness though, on the other side of the coin, there are examples like thalidomide and others which have been studied, licensed, put on the market and subsequently scientifically proved to be quite harmful.

DR. RITTER: That's correct.

THE CHAIRMAN: Would you not agree?

DR. RITTER: That's correct.

MS. CRONK: Q. And lest the import of the questions that I have put be misinterpreted, I am

suggesting to you, Dr. Ritter, that that is the point of the exercise of a public health agency, that it's important that you respond to and investigate any suggestions of that kind to determine whether in fact they are validly and soundly based?

DR. RITTER: A. Yes, I would just add the caveat, plausible suggestions.

Q. Yes. And this was a case where it was plausible, it was investigated and, based on the work that has been done, it has been found to be unfounded or scientifically invalid; would you agree?

A. That's correct.

Q. Thank you. And then just one final aspect of that dealing with fenitrothion, one of the insecticides specifically tested in the study that you did with Dr. Ecobichon.

I asked you yesterday in a totally different context whether there were any applications which had led to approval of a registered pesticide in this country intended purely for forestry use application, you recall that?

A. Yes.

Q. And I believe you responded to me that fenitrothion was an example of one of those?

A. Yes.

Q. Am I -- in thinking about that further last evening, Dr. Ritter, am I correct that at the time that the registration for fenitrothion in Canada was granted, it had a very extensive agricultural use experience international?

A. That's correct.

Q. Across the world?

A. That's correct.

Q. And that there was a considerable amount of data concerning both its health effects and other aspects of its usage arising from that agricultural usage experience?

A. Yes.

Q. There was an existing database?

A. Yes.

Q. And is it fair to say that that existing database formed part of and was certainly available for the purposes of the evaluation -- registration evaluation process here in Canada?

A. Yes.

Q. All right. It would, I suggest to you, be inappropriate and erroneous - I don't suggest that you suggested this - but it would be inappropriate to conclude that all of the work which you have described to the Board need be done to put together a

registration package in this jurisdiction was done for fenitrothion simply for the purposes of a forestry use application?

A. That's correct.

Q. Thank you. Dr. Ritter, those are my questions of you and I thank you.

Mr. Kingsbury, I wonder if I could turn to you now, sir, by way of a number of preliminary matters, and then I have some questions for you concerning the ESSA Document itself and certain of its content.

If I could deal with the preliminary matters first. You will recall, Mr. Kingsbury, that you spent some time yesterday morning explaining to the Board the environmental impact assessment strategy that is applied to forestry pesticides. Do you recall that?

MR. KINGSBURY: A. Yes, I do.

Q. All right. And one of the exhibits that you provided to the Board was Exhibit 711D which is a summary of that strategy as applied to forestry pesticides and you explained in detail what is involved in each of the items identified in that exhibit?

A. That's correct.

Q. All right. You referred a number of times as I took it down in my notes, Mr. Kingsbury -

and I perhaps didn't get it down accurately - but I thought you referred a number of times to "we" in the sense of: "we" used an indicator species approach, for example, "we" picked a species that we knew we were able to study upon which there was data available, examples of that kind. Do you recall that?

A. That's correct.

Q. All right. What I was unclear about, Mr. Kingsbury, and I would ask for your clarification: When you described to the Board the strategy applied to environmental impact assessments for pesticides -- forestry pesticides, as outlined on this exhibit, were you talking about the strategy that was employed -- that is employed by the federal government, that is employed by the Ministry of Natural Resources, that was employed by the ESSA group, or is one that you propose? Where does it fall in those possibilities?

A. I guess the most appropriate answer to what I meant by the royal "we" was the strategy that has been in place and has involved within the federation registration process in Canada. It would be a strategy that has come from the ongoing interaction of the various agencies that I spelled out who play a role in terms of assessing environmental risks.

Q. And was the work done by the group of

experts who produced the ESSA report consistent with that approach to environmental impact assessment?

A. The ESSA -- the people who worked on the ESSA Document basically reviewed a literature base that reflected the fact that that is the strategy in place in assessing environmental impacts of forestry pesticides.

Q. All right. It was then consistent with that approach?

A. Yes.

Q. Yesterday as well, a number of times in the course of your description to the Board of the result of the ESSA investigations concerning potential impacts, you described what you said were observed effects of chemical insecticides. Do you recall that generally?

A. Yes.

Q. By way of an example, one of the descriptions that I recall you dealing with was the observed effects of chemical insecticides on aquatic invertebrates in forest ponds and static waters and you compared for the Board what the effect of aminocarb was, carbaryl and fenitrothion in relationship to one another. Do you recall that?

A. That's correct.

Q. All right. Would you agree with me, Mr. Kingsbury, that the approved application rate for chemical insecticides in Ontario varies from chemical to chemical?

A. Yes, absolutely.

Q. So that when one attempts to make toxicity comparisons as among the chemical insecticide group it is important to recall and to understand that the authorized application rates of those chemicals do differ?

A. Absolutely.

Q. All right. And I guess what I am suggesting to you, inferentially at least, and now expressly, Mr. Kingsbury, is that in looking at the comparative impact of chemical insecticides in terms of their effect on any particular part of the environment one should with great caution make blanket or generalized toxicity comparisons because the chemicals are different?

A. I agree.

Q. As are the application rates and the circumstances of their use?

A. Yes.

Q. Thank you. Then yesterday -- following from your evidence yesterday as well there

was a discussion, you may recall, when you outlined for the Board what was meant by bio-concentration and bio-magnification as referred to and defined in the ESSA Document. Do you recall that?

A. Yes, I do.

Q. All right. There was one element of your evidence that I wished to have clarified. At one point, as I took it down in my notes - and I indicate immediately I may have not recorded it accurately - but I understood a question to be put to you by the Board to this effect: Are you saying that there isn't any, meaning bio-concentration or bio-magnification, or that you wouldn't register them if they did occur.

Do you remember the question, let me just put it that way?

A. Yes, I recall the question.

Q. All right. There were two elements to that question. The first is: Does it happen, and the second is: If it did, would they be registered.

As I understood your testimony yesterday you indicated, and I would ask for your confirmation, that with respect to the pesticides in use in Ontario there is no evidence that bio-concentration or bio-magnification occurs; is that correct?

A. Could you just repeat that so I am

clear?

Q. Yes. I understood your evidence to be yesterday that with respect to the pesticides used in forestry in Ontario in forestry situations, there is no evidence that bio-magnification or bio-concentration occurs. Did I understand your evidence correctly?

A. I believe so, although I may have dealt with those individually and I believe when I came back and talked to the Board and said I had used the royal "we" and talked about those, that I specified there is no evidence of significant bio-concentration and significant bio-magnification.

Q. All right. Well, that's the refinement of that answer that I am interested in, Mr. Kingsbury. You have added the word significant bio-concentration or bio-magnification because, as I understood it, you told the Chairman yesterday that if they existed, those compounds, those chemicals would not be registered?

A. That's very much the belief that -- I said to the Board that if there is evidence of significant bio-concentration or bio-magnification it would, in fact, lead to a product not receiving registration.

Q. Yes. And would you agree with me

that the important -- one of the important features of what you just said is the word significant?

A. I would agree in that we are dealing with a rather complex issue, and I didn't go into it at length yesterday.

When I say that, the idea of measuring bio-concentration in terms of the relationship as it is defined, as we are using the word here. And, once again, I remind the Board that I said we are dealing with what the ESSA Document says and beware of the fact that when we get out into a broader range of literature, these terms are sometimes used in a variety of ways.

Q. I understand.

A. We are talking about a relationship between residues in the environment and residues in an organism.

Now, in some cases, and particularly in lab situations, it's fairly easy to measure those. When we get out into a field situation residues -- you can measure residues in substrates, in foliage or soil or whatever it may be, and you can measure residues in organisms, but they are measured in different terms.

Obviously a part per million, which is a measurement of a weight of a pesticide to a weight of a

substrate, means something very different if you're measuring it in a soil than it might mean if you are measuring it in air.

Q. Could I stop you there just for a moment, Mr. Kingsbury.

A. Mm-hmm.

Q. And could we back up just a step.

A. Yes.

Q. The terms bio-concentration and bio-magnification were defined in the ESSA Document by the authors of that report for the purposes of their report; am I correct?

A. Right.

Q. And do I understand you simply to be alerting those of us who read the report and the Board that one has to bear the authors' definition in mind when you come to read the report?

A. That's right.

Q. All right. And that that definition may differ from that employed by others in other endeavors?

A. Absolutely. Not only in the literature, but in the protocols and in regulatory things like the federal register.

Q. All right. My only point to you, Mr.

Kingsbury, is this that is it fair to say that the fact per se of evidence of bio-concentration or bio-magnification will not of and in itself prevent registration or cause deregistration, if I can put it that way?

The fact of it per se isn't enough to do that, it's a much more complicated question; isn't it?

A. If you are dealing with the fact per se to mean that at one point in time in one study one shows that the concentration -- you have a value in an organism that, as best you can measure, reflects a higher value than what's in the environment.

Certainly this is an area where a number of things have to be taken into account and one of the major things is what happens over time.

Q. All right. And the point is that it must be significant bio-concentration or bio-magnification?

A. That's right.

Q. All right. And there is a whole range of parameters that have to be looked at; is there not, including things such as the toxicokenetics, the rate of elimination, the excretion characteristics, the metabolism characteristics, the breakdown, the time frame, all of that is part of a consideration of

whether bio-concentration is significant or not; would you agree?

A. Absolutely, and I could add to the list with things particularly relating to the nature of the exposure.

Q. Thank you. It has been clarified for me, thank you. Could we turn then particularly to the ESSA Document and I would like for the moment to deal with the subject of possible impacts of pesticides in forestry use situations on wildlife.

And as I understood your evidence with respect to those types of impacts, impacts on wildlife and indeed any potential impact, they were really considered in two contexts; that is, the potential for direct effects and the potential for indirect effects; is that correct?

A. That's correct.

Q. Dealing for the moment just with herbicides, I would like to review with you and ask for your confirmation or clarification, as appropriate, of what I understand the conclusions expressed in the ESSA Document to be with respect to the impact of herbicides on wildlife.

Am I right, first, that it was the conclusion of the ESSA review team that it was most

unlikely that herbicides have a direct adverse toxic effect on wildlife?

A. That's right, and I agreed with that conclusion.

Q. All right. Am I also right that following from the first conclusion it was the conclusion of the ESSA review team that it was, therefore, unlikely that herbicides have an adverse effect on prey animals; that is, the food source for carnivorous wildlife in the forest?

A. That's included in the conclusion, but that doesn't say that some prey animals might not be impacted by indirect effects.

Q. All right. Could I take you to page 63 of the ESSA Document, please.

A. Yes.

Q. Dealing with the issue of direct significant toxic effects, I direct your attention to the conclusion expressed at the bottom of page 63, and is it fair to say in reading through the ESSA Document that it was the conclusion of the review team that herbicides are unlikely to have a significant toxic effect on prey animals?

A. That is correct.

Q. All right. And the reviewers also

considered, as you point out, whether herbicides had an indirect effect on wildlife and, in particular, they looked at the question of possible impact on wildlife habitat; am I correct?

A. That's correct.

Q. And there's quite a lengthy discussion in the ESSA Document on that issue; is there not?

A. There is.

Q. All right. Well, I would like to deal with that issue for the moment, impact on wildlife habitat, and I would like to try to summarize what I understand the conclusions by the reviewers to have been, and perhaps the best way to do this is actaully go through the report.

Could I ask you, if you would, to start please at page 68.

A. Yes.

Q. Dealing with the first full paragraph after the indented paragraphs, am I correct that the authors suggest that any direct evidence of the effects on wildlife of habitat changes resulting from herbicide use is extremely limited?

A. That is what the authors report, yes.

Q. They also suggest; do they not, that

the available evidence is speculative - that's their word - based on presumed changes to habitat resulting from herbicide use?

A. That's what they report, yes.

Q. All right. And if we go over to the neck page they suggest; do they not, and conclude or at least point out that there are other forest management activities apart from the use of herbicides that have a far greater impact on habitat than does the use of herbicides itself?

A. Yes.

Q. An example given by the ESSA reviewers, for example, is the use of mechanical methods to control competition?

A. Mm-hmm.

Q. Is that a yes, sir, for the record?

A. Yes. Sorry, yes.

Q. All right. Do the authors suggest as well at page 69 that such effects as there are from the use of herbicides on wildlife habitat are the result primarily of the herbicides actually being effective for their intended purpose?

A. That's correct.

Q. All right. And stated that way, perhaps it is self-evident, but am I right that the

point of that is that one of the primary uses in forestry of herbicides is for the control of competing vegetation, competing plant species?

A. Yes.

Q. And, therefore, the intended effect is to actually effect a change to vegetation?

A. Absolutely.

Q. Vegetation forming part of the wildlife habitat in the forest?

A. Yes.

Q. So that, therefore, inherently there is a change to wildlife habitat occasioned by herbicides because there is a change to vegetation?

A. That's correct.

Q. All right. But that is really only the preliminary step in assessing the significance of the change as to whether it's positive or negative and to what extent that it occurs?

A. It is the preliminary step and also I think, as I pointed out, because there is a lack of direct toxic effects, it is in fact the critical step.

Q. All right. And the point the ESSA experts were making; is it not, is that alternatives to herbicides use having the same purpose; that is, control of competing vegetation, may in fact have

greater impacts on habitat? The alternatives may have a greater impact than herbicides would?

A. Just clarify for me alternatives in terms of...?

Q. Competition control. The alternatives available--

A. Yes.

Q. --to the use of herbicides may have a greater impact on habitat than herbicides will?

A. They may have a greater impact. They are almost certain to have similar impacts in that if in fact they are as successful at changing the plant community.

Q. Yes, thank you. Too equally effective alternatives; one being herbicides, one being another forest management technique. It may well be, the authors suggest, that the non-herbicidal treatment may have greater effects on wildlife habitat; isn't that so?

A. Yes. I wouldn't put great emphasis on the greater. Certainly there is a -- in terms of comparable, and maybe I'm just not -- I'm not sure that that's supported by what is said here in terms of greater effect.

Q. All right. Could I ask you to go --

not much turns on it. I thought it was a basic principle, Mr. Kingsbury.

A. Okay.

Q. But could I ask you to read to yourself for a moment, please, the top of page 70?

A. Okay. That's correct. As you have pointed out, alternatives may have greater -- even greater impacts on the habitat of some species.

Q. Thank you. And is it also not true that the degree to which there is an impact by the use of herbicides clearly will be a direct result of the extent of the area treated and the manner of treatment?

A. Absolutely.

Q. All right. And that there are some species -- depending on the home range of an animal species, there may be little or no effect from the use of herbicides to that species or the converse may be equally true?

A. That's true.

Q. All right.

A. If I may, I think I'm sort of thinking backwards here. I believe some of the problem I had with the even greater impact is I was thinking specifically of alternative tending methods but, as I pointed out, I believe a number of times yesterday,

looking at other timber management activities, yes, I would very much concur that there were even greater effects from some of the other activities and, as I spelled out, they are done in sequence with tending.

Q. All right. And am I also correct by way of final summary that the ESSA authors or reviewers conclude in general that herbicides, as they are used now in forestry, do not have a serious -- will not seriously endanger wildlife habitat; that's their general conclusion?

A. I think that the authors have perhaps not made that conclusion that they will not seriously alter wildlife habitat so much as they have said that they will modify wildlife habitat and that will be different in terms of whether it's a positive or a negative or a negligible effect depending on the habitat requirements of different species of wildlife.

Q. Mr. Kingsbury, could I ask you to go to page 71, please, to the first sentence in the first full paragraph.

And I suggest to you, sir, that what the ESSA reviewers expressly found was that, in general, it would appear, based on their review, that herbicides as they are used now implicitly in forestry will not seriously endanger wildlife habitat?

A. That's what it states there, yes.

Q. That was their general conclusion; was it not?

A. It certainly is written here as a general conclusion in their discussion, yes.

Q. All right. And, in fairness to you, what you have said was that you have to examine the nature of the change to determine whether it's positive or negative and that will vary according to species?

A. Yes. And I will note that in making that general conclusion they have very appropriately begun to talk about the things that you mentioned as being important in terms of other changes and the size and scale of herbicide-induced changes.

Q. All right. And the authors then go on - I don't propose to go through each example, Mr. Kingsbury - but the authors then go on to point out a number of situations where habitat changes in fact effect positive changes to habitat; isn't that so?

A. Very much so, yes.

Q. All right. And one of the issues or changes much discussed in this report is the effect on browse for moose and deer?

A. Yes.

Q. And a number of reports are referred

to by the ESSA reviewers which suggest that there are either -- which suggest that there is a beneficial impact from the use of herbicides on deer and moose browse materials; isn't that so?

A. At some points in the time cycle of blank community response, yes.

Q. All right. Well, for example, the reviewers refer to an article published by Sullivan, et al, in 1985 which demonstrated a preference by deer for glyphosate-treated browse; isn't that so?

A. They did cite that study; yes, that's correct.

Q. Am I correct, Mr. Kingsbury, that that study is contained in a textbook entitled: The Herbicide Glyphosate, published by Butterworth in 1985?

A. That's right, and in fact there is a chapter in that book authored by Sullivan.

Q. And it's from that chapter that the reference is made, or with respect to that chapter; isn't that the case?

A. That's correct, and it's also published in Canadian Journal of Zoology, I believe.

Q. All right. Are you familiar with this textbook, Mr. Kingsbury?

A. I'm familiar specifically with the

portions of that textbook relating to impacts on organisms and I'm also familiar, to a lesser extent, with other parts of the book, yes.

Q. All right. Are you familiar with the portion in the text dealing with the state and biological consequences of glyphosate in the aquatic environment?

A. Yes.

Q. All right. Would you agree with me or can you tell me whether this book is recognized as an authoratitive text on the subject of the herbicide glyphosate?

A. I believe it's one of the most recent and comprehensive reviews of the material, yes.

Q. Thank you. Then apart from the Sullivan work which is referred to in the ESSA Document on this issue of impact on browse for moose and deer, the ESSA reviewers suggest with and support their suggestion with reference to a number of studies that better foliage may be available following herbicide treatment as new growth is introduced; isn't that so?

A. Yes.

Q. All right. And one of the articles referred to in the ESSA Document at page 74 in support of that suggestion -- well, there are several. One is

by Sullivan, 1985, we have already discussed that. One is by Crefting and Hanson, 1969; am I correct?

A. Could you just...

Q. Second paragraph, page 74.

A. Second paragraph, okay. Yes, that's correct.

Q. There are a number of authorities referenced there?

A. Yes.

Q. A number of studies, Crefting and Hanson 1969, Baroco, et al, 1972. Do you see that?

A. Yes.

Q. Sullivan and Sullivan, 1980 unpublished data; do you see that?

A. Yes.

Q. And the fourth is an article by Newton, et al which is described as being in press; is that correct?

A. That's yes.

Q. I'm going to show you an article, Mr. Kingsbury, a copy of which I earlier provided to you entitled: Fate of Glyphosate. Actually that is the wrong article, excuse me. Could I just have a moment, please.

Entitled: Browse Availability After

Conifer Release in Maine's Spruce/Fir Forests by Michael Newton and four other authors including an M. L. McCormick, Jr. Are you familiar with the article?

A. Yes.

Q. Is this the article that is referred to in the ESSA Document as being in press?

A. That's my belief. Yes, that it is.

MS. CRONK: That will be the next exhibit, please.

THE CHAIRMAN: Exhibit 722.

---EXHIBIT NO. 722: Article entitled: Browse Availability After Conifer Release in Maine's Spruce/Fir Forests by Newton, et al.

MS. CRONK: Q. This article, as I understand it, Mr. Kingsbury, is to be published. If it was not published in July of this year, it's to be published in August of this year in a journal entitled: The Journal of Wildlife Management, a U.S. publication. Is that your understanding?

MR. KINGSBURY: A. The article is in fact published.

Q. It is published.

A. With the citations there.

Q. And I take it that the ESSA reviewers had available to them a pre-print copy of the document?

A. Yes.

Q. All right. And I would refer you to the abstract reference to the paper. Am I correct that in this particular study a variety of herbicides, including glyphosate, were applied seven years after harvest to a spruce/fir forest in the northeast United States?

A. That's correct.

Q. And according to the abstract description of results, the study indicated, as a result of the application of glyphosate that a four to eight fold increase in available browse resulted?

A. That's correct.

Q. And, further, that nearly all plant species present before the herbicide treatment remained some nine years later with a greater percentage of sungrown foliage and twigs than existed in the untreated plots; is that correct?

A. That's correct.

Q. In essence, the study suggests that nine years after treatment of a forestry site with glyphosate, browse on the sprayed plots was significantly better than on the untreated plots; isn't that so?

A. Not only that, but it I believe

demonstrates that for sites sprayed with a variety of herbicide materials.

Q. Including...?

A. Including glyphosate.

Q. Yes, in the phenoxy herbicide category; am I correct?

A. Right.

Q. All right. And if we go to page 644 very briefly in the first full paragraph under Table 1--

A. Mm-hmm.

Q. --I suggest that the nature and the purpose of the study is outlined and it's indicated as outlined in the abstract that:

"Several herbicides in the phenoxy category were experimentally applied for conifer release seven years after harvest with the objective specifically of recording vegetation development that followed and to investigate and relate that development to browse availability."

That was the purpose and the nature of the study; is that correct?

A. Yes.

Q. And, in general terms, just to move

through it quickly if we can, am I correct that the results were analysed one year after treatment and again nine years after treatment?

A. Yes.

Q. And am I correct that the study supports the earlier literature referred to in the ESSA Document that browse may be improved following herbicide application if one of the herbicides is in the phenoxy group?

A. Yes.

Q. And the conclusions by the authors are set out over at page 648. I would ask you to go there for a moment, if you would.

I direct your attention to the very last paragraph. Can you confirm for me, Mr. Kingsbury, that apart from the conclusions you have already indicated to the Board were reached by the authors, there is also an indication by the authors that, to the best of their knowledge, none of the herbicide treatments eradicated any plant species?

A. That's correct.

Q. Further, the tall cover was not eliminated rather simply reduced with ratios of cover types changed?

A. That's correct.

Q. All right. I take it from that that all plant species remained following treatment although the compositions of the plant species or the extent of each plant species may have differed? Is that a fair reading of it?

A. That's the way I would read it, yes.

Q. All right. Do you know Professor Michael Newton either by reputation or personally?

A. Yes.

Q. What is his position or involvement in forestry?

A. He's had a longstanding involvement in forestry field trials in the United States.

Q. Is he an acknowledged expert in your opinion concerning matters related to forest management practice and, in particular, the use of pesticides with particular emphasize on herbicides?

A. Yes.

Q. And are you -- do you know either personally or by reputation Professor M. L. McCormick Jr., of the College of Natural Resources, University of Maine, Orno?

A. Yes, I do.

Q. And, again, what is the area of his involvement in forestry?

A. Once again, considerable and extensive experience within the area of herbicide use and applications and the effects in forestry situations.

Q. And do you regard him as an expert in those areas?

A. Yes, I do.

MS. CRONK: I am sorry, sir, I neglected to know the exhibit number for that exhibit.

THE CHAIRMAN: It was 722.

MS. CRONK: Thank you.

THE CHAIRMAN: Mr. Kingsbury, comparisons have been made in some of this literature between the use of herbicides and other man or human treatments in terms of forestry practices; site preparation, mechanical and otherwise, this kind of thing.

What about comparisons between the change in or effect on habitat of some natural occurrences such as fire vis-a-vis the impact on changes in habitat due to fire as opposed to impact or changes on habitat due to the use of pesticides?

I guess what I'm getting at here: The areas affected would be infinitely greater in terms of habitat affected by non-human interference with the forest.

MR. KINGSBURY: I guess --

THE CHAIRMAN: Most of the comparisons seem to be made with human interferences.

MR. KINGSBURY: That's correct and I guess -- I believe you probably mentioned the critical aspect of the non -- the natural events that impact on wildlife habitat in the forests such as fire, then of course pest and disease activities, is that their occurrence is something that, in terms of a spacial and a temporal fashion, it happens when it happens and wildlife simply has to respond.

And if it's a large fire, obviously, it can have a very negative effect over a very large area, or if it's something like, you know, a historical budworm infestation as we know from the forest record, prior to human intervention into that that vast areas may all of a sudden drastically change in terms of the wildlife habitat that they preserve.

THE CHAIRMAN: So could you reasonably conclude that whatever the changes might be due to the use of herbicides, and even some of those changes on habitat, may be positive in the long run--

MR. KINGSBURY: Yes.

THE CHAIRMAN: --they would be minor in terms of amount of habitat affected by including, in

addition to human interference, natural interference?

MR. KINGSBURY: Absolutely.

THE CHAIRMAN: Or naturally-induced changes to habitat?

MR. KINGSBURY: That's correct.

THE CHAIRMAN: Thank you.

MS. CRONK: Q. Mr. Kingsbury, as I understand it, one of the other very relevant issues in the whole consideration of the assessment of risk from the use of herbicides is the issue of the persistence in and effect of herbicides on soils and water as well as directly on animals; am I right?

MR. KINGSBURY: A. That's correct.

Q. All right. And in terms of direct effect on animals, direct impact on wildlife, we have already reviewed what the ESSA team of experts concluded in that regard?

A. Yes.

Q. Am I also correct that the data requirements of relevant federal regulatory agencies in this country require specific studies to be prepared and submitted in support of a registration application dealing with such issues as degradation of a pesticide in soils, mobility, persistence not only in soil but also in water, in sediments?

A. Not only specific studies -- I'm not sure whether you have included, a petition for forestry registration would in fact require data of that type specific to the forestry sites and use patterns intended.

Q. All right. And that is a specific requirement I believe of Environment Canada as one of the involved reviewing agencies; isn't that so?

A. That's correct.

Q. All right. And as well the data regarding the specific metabolism of the chemical is required?

A. Yes.

Q. Are you familiar, Mr. Kingsbury, with an article published in 1984 by Professor Newton to whom you referred a few moments ago together with others entitled: The Fate of Glyphosate in an Oregon Forest Eco-system?

A. Yes, I am.

THE CHAIRMAN: Exhibit 723.

MS. CRONK: (handed)

---EXHIBIT NO. 723: Article entitled: The Fate of Glyphosate in an Oregon Forest Eco-system by Newton, 1984.

MS. CRONK: Q. Mr. Kingsbury, for our

ease of discussion with respect to this document, could I ask you just for a moment to number each of the pages. They are numbered from the publication but many of them didn't come out in the photocopying and I have it that there is eight pages, one being the cover page.

MR. KINGSBURY: A. Done.

Q. All right. Mr. Kingsbury, it is my understanding that this article by Professor Newton and his colleagues is regarded really as a classic published study of the investigation of a particular herbicide in a forest eco-system, particular in this case to glyphosate, but the study itself is well known and is considered a classic in its field. Would you agree with that?

A. Yes, I would.

Q. All right. The purpose of the study and the study objectives are set out on page 1 and I would refer you in this regard to the second -- well, the first paragraph above the section entitled: Experimental section. Do you see that?

A. Yes.

Q. And am I correct that the study objectives, as outlined by the authors, were specifically to determine first glyphosate deposits in various strata of forest vegetation after aerial

application and its persistence in those strata and in litter and in soil."

A. Yes.

Q. That was one of the objectives?

A. That's right.

Q. And, secondly, to determine glyphosate concentrations in stream water, sediment and fish after direct application to open streams?

A. That's correct. It's direct application in the context that we discussed it, as aircraft directly flying over the aquatic system being studied.

Q. All right. And, thirdly, the third objective was to determine glyphosate exposure and retention levels in various forest mammals?

A. That's correct.

Q. And, finally, the occurrence and persistence of the major metabolite of glyphosate known as AMPA, that is the acronym for it?

A. That's right.

Q. And to determine the occurrence and persistence of what is known as NNG a trace impurity in glyphosate; am I correct?

A. That's correct.

Q. All right. And, again, in an effort

to summarize the nature of this study and the work that was done, am I correct that there were two study sites involved; the first is as described by the authors as being the primary study site and the second, logically enough, the secondary study site?

A. That's correct.

Q. And am I correct that the first site was an eight hectare unit situate in Oregon over which a small stream flowed through the site lengthwise and on which existed two large shallow beaver ponds?

A. That's right.

Q. Apart from other topographical features.

A. Mm-hmm.

Q. And am I correct that the study sets out both the nature of the pre-treatment sampling, the nature of the treatment application, meaning the way the glyphosate was applied, and also the nature of the post-treatment sampling that was carried out?

A. That's right.

Q. And would you agree with me that both the pre-treatment and the post-treatment sampling were considerable for the purposes of this study?

A. Yes.

Q. Dealing just with the pre-treatment

side of it, the article suggests that samples were taken of vegetation, of soil and of -- of soil both covered and uncovered and of litter before any application of glyphosate?

A. Yes.

Q. Stream water was also sampled as were various terrestrial animals prior to the application of any herbicide?

A. Yes.

Q. And by the sampling of various animals they essentially collected various specimen species types from the site before the application of glyphosate?

A. Yes.

Q. And then two loads of glyphosate were applied at the rates described on page 2 as we have numbered them? There were two applications of glyphosate? I'm looking under treatment applications.

A. Yes. Just to clarify, when it says two loads, I believe that is two helicopter loads, that in fact.

Q. One application then?

A. There was one application. The treatment rate was 3.3 kilograms per hectare and I just note that. I believe that is about one and a half

times the maximum rate registered in Canada.

Q. All right, thank you. So there was one application at rates in excess of what would be permitted here?

A. Yes.

Q. All right. And then post-treatment, again, a number of specimens were taken and am I correct they included both aquatic and terrestrial samples?

A. Yes, and a number of substrates as I described that in both of those categories.

Q. And, in addition, both terrestrial and animal samples were collected actually at the time of spraying and at set intervals thereafter?

A. Yes.

Q. And litter samples, water samples and stream sediment were sampled?

A. Yes.

Q. Then, the secondary study site, as I understand it, was introduced to the study largely because shortly after the application of glyphosate on the primary site there had been a very unanticipated rainfall?

A. Yes.

Q. And they had 21 millimetres of rain

within 36 hours, as I understand it?

A. That's right.

Q. And that gave rise to the possibility of washoff?

A. One would think so, yes.

Q. A lot of rain. And, therefore, the second site also in Oregon was selected and aerial application at the same rate of glyphosate and the same volume of delivery with the same pilots and same equipment was used?

A. Yes.

Q. All right. And then at the bottom of page 3 we find the detailed description set out of the results obtained from both sites during the course of the study and over at page 6, as we have numbered it, on the bottom of the left-hand column we find the beginning of the discussion; is that correct?

A. Yes.

Q. All right. Could I ask you first, if you would, to outline for the Board what conclusions were formed by the authors based on their work regarding the metabolism of glyphosate, and I would refer you in that regard to the discussion beginning the first two sentences at the bottom of the column on the left on page 6?

A. Sorry, under metabolites and contaminants.

Q. Sorry, under discussion.

A. Yes, okay.

Q. Could you indicate to the Board what the observations or conclusions were of the authors regarding the metabolism of this chemical based on this study?

A. Okay. The authors concluded that glyphosate disappeared rapidly from this forest system, that it did not move into water from soil and, in fact, that much of the material degraded in the substrates where it landed which, in this study, was primarily in the vegetation.

Q. All right. And are those findings consistent with your understanding regarding the metabolism of glyphosate?

A. Very much so, yes.

Q. All right. And I direct your attention as well to the observations made at the bottom of page 6 in the right-hand column which read:

"The findings of absorbed glyphosate in stream sediment are of interest to the interpretation of soil data."

A. Yes.

Q. "The ability of sediments to glean moderate amounts of glyphosate from water when those sediments have much higher glyphosate concentrations than water corroborates earlier reports that particulate matter has a high absorptive capacity for glyphosate."

A. Yes.

Q. Do you see that? Now, stopping there for a moment, Mr. Kingsbury. Again, from my perspective to try to approach it from a layman's perspective, I understand that what this study demonstrated as a result of these field tests was that glyphosate, when it metabolized, in fact was a attracted to and absorbed to sediment, to particulate matter in water and did so in a way which led the authors to conclude that it does not -- in fact that its mobility in water was low.

Is that a fair summation of what the implications of those observations were?

A. I believe that you indicated when it metabolized that it basically became bound to sediments in water.

My interpretation is that the glyphosate itself, not simply glyphosate that had been

metabolized, i.e., undergone some degradation, became strongly attached to the sediment in the aquatic systems.

Q. Apart from that amplification, is the rest of what I suggested to you a fair--

A. Yes.

Q. --and accurate reflection of what the study found.

A. Yes.

Q. I'm sorry?

A. Yes.

Q. That has; does it not, implications for the issue of the potential for contaminant transport of this chemical in water?

A. Yes, it does.

Q. And as to the potential for a contamination of water sources both surface and groundwater?

A. It would imply that such contamination is dramatically limited.

Q. All right. And I would ask you in that regard if you would, please, over on page 7 to look at the further observations of the authors in this regard. And I'm still looking at the first paragraph at the top of the page there where the authors

indicate:

"We did not identify retention by sediment soil fraction. Should retention in sediments be studied intensively, techniques for separating soil into organic and inorganic compounds would be needed. Nevertheless, the chemical showed a high afinity for solids even when clean water was running through in substantial excess. The finding of roughly comparable AMPA levels in sediments and uplands soils suggests that residues are virtually immobile while being metabolized even in the presence of substantial leaching opportunity."

Does that support the suggestion I made to you a few moments ago, Mr. Kingsbury, that the findings of this study have direct implications for the potential contaminant transport of glyphosate in water?

A. It is in fact the findings on which that contention is supported.

Q. All right. And they are indicating; are they not, that even in the presence of clean running water the characteristics of the chemical were such that it bound to sediment, to soil particles as

distinct from being attracted to water, if I can put it that way, moving through the water?

A. That's right. Even when clean flowing water was running by, the material clung to the sediments.

Q. All right. And still dealing with the metabolism characteristics of the chemical, could I ask you to look to the last paragraph in the right-hand column on page 7 which indicates:

"Rates of degradation observed in foliage and soil corroborate the manufacturer's general statement based on agricultural data that glyphosate is broken down quickly."

Stopping there. Is that your understanding of the study results?

A. Yes.

Q. And do you accept them as being reliable in the face of the results that they obtained?

A. Yes.

Q. And further the authors indicate:

"Conditions of degradation here are likely to have been quite different from those encountered in agricultural studies suggesting a relative insensitivity of

glyphosate degradation to environment influence and a tendency toward rapid disappearance from both simple and complex eco-systems."

Do you agree with that conclusion?

A. Yes.

Q. And what do you understand the term a relative insensitivity of glyphosate degradation to environmental influence to mean?

A. I would interpret that to mean that the degradation of glyphosate is not based on a very narrow range of parameters; that in fact there are broad pathways that tend to operate in virtually all simple or complex eco-systems that would break this material down.

Q. And then dealing with the -- dealing further with the implications of the study for the movement and effect of glyphosate on water resources, could I ask you to go back to the left-hand column on page 7.

A. Yes.

Q. And look specifically this time at the first full paragraph beginning with the words:

"The absence of glyphosate..."

Do you see that?

A. Yes.

Q. All right. And am I correct that the suggestion made by the authors in that paragraph and the immediately following paragraph is that based on the tendencies of the chemical which they observed, that contamination of water -- well, first, they found that any contamination of water by direct spray application was within the range of safety to stream plants and irrigated crops suggested in the scientific literature. They did not find excess contamination?

A. That's right.

Q. And, further, am I correct that they concluded based on the data which they actually measured from these tests that there was no mutual risk from including forest streams within the spray pattern as a result of using this chemical?

A. That's right.

Q. And, further, that there was no effect on water quality from devegetation resulting from the spraying until the following year?

A. That's right.

Q. At which time they indicate dead shade would persist?

A. That is their conclusion, yes.

Q. All right. That conclusion speaks to

the issue of aquatic habitat; does it not, vegetation on the shores?

A. Indirect effects, yes.

Q. All right. And what is the implications of saying that a year later when effects were observed dead shade would persist?

A. They are basically saying that the modification of the aquatic habitat at that time would be limited, because under -- in this site and the way they treated it there would still be considerable cover over the stream from vegetation that had been killed but which still presented a barrier to sunlight penetration. That is the way I would interpret it.

Q. And then, Mr. Kingsbury, they conclude finally; do they not, based on the data which they collected and their observations that they anticipated negligible effect overall of glyphosate on forest water quality? That was their conclusion; was it not?

A. That's correct.

Q. And do you their share their view based on the data evidenced in this report?

A. I would say that in this report they are anticipating an effect, that the study in itself is not sufficient to provide proof of that effect, but I

would say that they have gone a very considerable distance to eliminating probable causes of the effect.

Q. And do you support the directions suggested by their conclusion and; that is, that their data supports the view that it is unlikely that there would be significant negative effect to forest water quality from the use of glyphosate?

A. I would very much do that. Of course, I have the advantage of hindsight in that other studies, and thinking particularly of the Carnation Creek experiment, which would have directly generated data on many of the things that contribute to the direction they are talking about confirms that that, in fact, has largely been the case.

Q. All right. So subsequent research corroborated the suggestion they were making back in 1984 based on their data?

A. That's correct.

Q. All right. And do you then agree today with the conclusion that they are suggesting in this paper; that being, that there will be negligible effect overall of glyphosate on forest water quality in the circumstances of an aerial application of this kind?

A. That's correct.

Q. All right. The study also considered; did it not, the impact on wildlife of the use of glyphosate on both of these study sites?

A. Yes, it did.

Q. And am I correct that, if I could put it in general terms, that based on the data which they collected, they concluded that the toxicological risk from forest use of glyphosate was probably, in their terms, zero for either wildlife or humans?

In essence, they were saying there was very negligible risk?

A. That is their conclusion, yes.

Q. All right. And they based that conclusion; did they not, on their observation of low exposures and rapid elimination of the chemical in the animals that they sampled?

A. Yes.

Q. And that was so notwithstanding or regardless of any feeding preferences of the wild animals they tested?

A. That's correct.

Q. Would you agree with the suggestion made by Professor Newton and his colleagues that those two features afford reassuring evidence that wildlife and their food supplies are unlikely to be threatened

by glyphosate?

A. Yes.

Q. The fact that glyphosate is rapidly eliminated is particularly significant in that context; is it not?

A. Certainly, as we discussed in talking about direct effects in toxicology just as environment, once you no larger have exposure you no longer have possibility of effect. So, yes, I agree the fact that it rapidly disappears and eliminates potential for further effect, direct effect.

Q. Well, in fact, just as an example of that principle and conclusion, the authors indicate that with the small mammals that they tested,

"...detectable intake of the chemical was limited to the first month after treatment and the elimination systems maintained body levels low enough to suggest that the non-visceral parts were a part of the elimination pipeline."

A. That's correct.

Q. I am quoting from the article.

A. Yes.

Q. And, in fact, when you look at the actual residues which they measured following treatment

the highest contents were in the stomach; were they not?

A. Yes.

Q. That suggests direct ingestion?

A. Yes.

Q. As opposed to accumulation in the body?

A. That's right.

Q. So, in essence, if I can summarize that, as I understand, the data which the authors collected demonstrates and supports, first, a rapid elimination rate of the chemical from animal systems?

A. Yes.

Q. Secondly, a negligible effect on water quality?

A. Yes.

Q. Thirdly, a rapid degradation pattern?

A. Yes.

Q. Fourthly, an infinity of the chemical in terms of its metabolism for sediment as distinct from water?

A. Yes.

Q. And that there -- and, fifthly, negligible toxicological risk to either wildlife or humans from the use of the chemical?

A. That's right.

Q. And, finally, with respect to this study, you will recall that one of the objectives, the stated objectives was to detect the occurrence and persistence of what I termed NNG which, as I understand it, is nitrosylglyphosate - am I pronouncing that correctly? In any event, it is a trace impurity in glyphosate?

A. That's my understanding, yes.

Q. It is known as NNG?

A. Yes.

Q. And, further, to similarly detect the occurrence and persistence of AMPA, the major breakdown or the major identifiable metabolite of glyphosate?

A. That's correct.

Q. And if we look over at page 6 of the study, would I be accurate in suggesting that the scientists failed to find any AMPA in any animal tissue after 14 days following herbicide treatment?

Sorry, I am looking at page 6 the column on the left.

A. Yes.

Q. It is paragraph --

A. I'm just... That's correct.

Q. All right. And they concluded from

that that the chemical was eliminated rapidly and absorbed in negligible amounts?

A. Yes.

Q. All right. And with respect to NNG, am I correct that none of the fish collections which they tested showed measurable whole body levels of glyphosate, AMPA or NNG at a detection limit of 0.05 - is that milligrams per kilogram of body weight -

A. That's correct.

Q. --despite glyphosate concentrations in stream water that remained in the detectable range for three days?

A. That's correct.

Q. All right. In essence they didn't find any NNG of concern; is that correct?

A. In fish they failed to show NNG or AMPA or glyphosate.

Q. All right. And the AMPA that they found in other species and animal tissues was found to eliminate very rapidly; is that correct?

A. Yes.

Q. Mr. Kingsbury, that's been a rather long way of reviewing this article, but would you agree with me if I suggested to you that the data presented in this paper, coupled with the later research outlined

in the ESSA Document, confirms that there is very little, if any, environmental risk afforded in the forest eco-system to the use of glyphosate in forestry applications?

A. In terms of the direct toxilogical risks and the risks that come from persistence of glyphosate residues in the environment, yes, I would agree.

Q. All right.

A. I would still note that there are indirect effects through the desired action of glyphosate that for some species, at some point, might be considered by some people to be negative effects.

Q. I wasn't suggesting there was no effect at all, but rather that it was not adverse in terms of environmental impact?

A. That's correct.

Q. All right. And with respect to the issue of glyphosate in fish, I asked you earlier if you were familiar with the chapter in the Grosbard and Atkinson text entitled: The Herbicide Glyphosate that deals specifically with the effect of glyphosate on aquatic organisms, and I believe you told me you were?

A. Yes.

Q. I would like to read a short passage

of that text to you and ask you whether you agree or disagree with it, Mr. Kingsbury.

I am reading from page 216 in the conclusion section of this chapter. It reads as follows:

"Glyphosate, when used as recommended by the manufacturer, is unlikely to enter water courses through run-off or leaching following terrestrial application. Clearly, significant residues could enter water when Roundup is used as an aquatic herbicide."

Stopping there for a moment, that's if it is applied directly into the water?

A. That's correct, and...

Q. "In static water, residues decline fairly rapidly due mainly to absorption onto particulate matter. In some flowing water the conditions could be such that glyphosate could remain in solution or absorbed onto particulate matter in suspension for some distance downstream from the point of application. Although the rate of degradation of residues depends on pH, temperature and

the presence of micro-organisms, most aquatic environments likely to receive Roundup treatment will provide the most ideal conditions for degradation."

Stopping there. Do you agree with that?

A. Yes.

Q. "The field experience seems to support the laboratory predictions that residues will be absorbed and will eventually break down."

Do you agree or disagree with that?

A. I agree.

Q. That is in fact consistent with the Newton study; is it not?

A. And other studies done subsequently, yes.

Q. "Therefore, it is unlikely that glyphosate will affect aquatic organisms at the concentrations found in the environment after use at the recommended rates."

Do you agree?

A. That's correct.

Q. And finally:

"It is also unlikely that residues will

be accumulated in fish tissues."

Do you agree?

A. I agree.

Q. All right.

MS. CRONK: And then finally, Mr. Chairman, I would like to mark as the next exhibit OFIA -- the response to OFIA Interrogatory No. 28T from Panel 13 which is a paper entitled: The Determination of Persistence, Movement and Degradation of Hexazinone in Selected Canadian Boreal Forest Soils.

THE CHAIRMAN: Exhibit 724. Ms. Cronk, can you indicate how much longer you might be?

MS. CRONK: I would expect to be ten minutes, sir.

THE CHAIRMAN: Okay. We will finish off then. Thank you.

MS. CRONK: (handed)

THE CHAIRMAN: Thank you.

---EXHIBIT NO. 724: Paper entitled: The Determination of Persistence, Movement and Degradation of Hexazinone in Selected Canadian Boreal Forest Soils.

MS. CRONK: Q. Are you familiar with this paper, Mr. Kingsbury?

MR. KINGSBURY: A. Yes, I am.

Q. It considers in the context of

hexazinone the same persistence, mobility, metabolism and degradation issues as were relevant to the Newton paper on glyphosate; is that so?

A. It deals with a narrower range, primarily the issues of persistence, mobility in forest soils.

Q. And one of the authors of this report was Robert Campbell who has previously testified in these proceedings?

A. That's correct.

Q. All right. What was the general nature and purpose of the study? Can you help me with that?

A. This was a soil -- or a study that was carried out under the impetus of the Institute I worked for to look at the effects in two different types of soil, in boreal situations in Ontario specifically, and for those two types of soil which were basically a clay and a sand site to see, (1) how hexazinone that was applied directly to that soil would persist, to see whether it would move both vertically downward within the soil profile and also horizontally down a slope after application.

Q. All right. And dealing with the results of the study, let's deal with mobility first as

you mentioned it last.

Can you outline for the Board what the conclusions were, based on the evidence presented in this test, regarding the mobility characteristics of this chemical?

A. The conclusions are basically that the material showed very little movement downward into the soil column and there was no indications of movement laterally. That reflected the fact that the first sampling point was, I believe, one or three metres from the edge of the treated area -- three metres, so it did not move three metres.

Q. And what were the conclusions based on the data emerging from this study regarding the leaching properties of this chemical?

A. Basically that there was very limited leaching potential for this chemical on either sand or clay sites.

Q. And, in your opinion, was that supported by the data which emerged from this study?

A. Absolutely.

Q. All right. And what were the conclusions reached regarding the degradation characteristics of the chemical?

A. That in fact the material showed a

degradation to -- to a half life, as we discussed it yesterday, in the order of 43 days in both sites and that the material continued to degrade from that time onward to very low levels.

Q. All right. Rapid degradation then?

A. It certainly is a -- rapid is a relative term. Certainly a comparable degradation rate to what is known for herbicides used in -- other forestry herbicides and herbicides in general in soils.

Q. And what were the conclusions of the study regarding the persistence of the chemical, or have you dealt with that in the context of talking about degradation?

A. Basically that it would not persist within these soil types, that it would in fact degrade.

Q. And do you agree or disagree with the conclusions expressed in this study, Mr. Kingsbury?

A. I would agree.

Q. Thank you. Mr. Kingsbury, there was just one other matter and it is really by way of clarification of what I understand your concluding remarks to have been to the Board.

I have consulted with my colleague Mr. Shibitani because we don't have a transcript to see whether his notes and mine were the same.

I understood you to tell the Board yesterday at the conclusion of your evidence-in-chief that it was your opinion, and I am quoting:

"The pesticides registered for use in forest management have negligible or limited impact on ecologically critical aspects of sensitive non-target communities."

Was that what you said to the Board? Was that your evidence?

A. I believe that's exactly what I said to the Board.

Q. All right. You will forgive me, Mr. Kingsbury, if I observe as a layperson that that's a rather complicated statement. I wish to ensure before your evidence is complete that I understand exactly what it is that you mean by that.

Do you mean by that statement that the pesticides that we are concerned with in this proceeding, as used in forestry applications in the forest, are safe for use in ecological terms?

A. I think that I am, in general, concurring with that in that I believe the way I have stated it is that using the assessment strategy that we employ to assess them we find that there are, in fact,

negligible or limited effects on the environment.

Q. And the parameters that you look to for assessment are those which are perceived to be most sensitive?

A. And they are the parameters that we believe are most reflective of overall effects on the environment, yes.

Q. And, as well, those that are considered to be the most sensitive?

A. Yes. In fact, that's one of the reasons we select those parameters.

Q. All right. Is it then your opinion, Mr. Kingsbury, that the pesticides in use or authorized for use in forestry in Ontario, having regard to the nature of the assessments that you have described, are safe from an ecological perspective if used properly in accordance with authorized procedures?

A. I guess, along with Dr. Ritter, I would have some concerns over just how we use safe. I believe I would state it as, they do not cause unacceptable environmental disturbance.

Q. And would you, like Dr. Ritter, concur that, given the fact of their continuing registration status, that members of the public are, insofar as your discipline is concerned, entitled to

rely on that registration status as evidencing that they are safe for use if used properly?

A. Yes, I would.

Q. Thank you very much.

MS. CRONK: Those are all my questions, Mr. Chairman. Thank you.

THE CHAIRMAN: Thank you, Ms. Cronk. Thank you, panel.

It is time to adjourn for the day. We will adjourn until next week at 1:00 p.m.

Mr. Colborne?

MR. COLBORNE: Mr. Chairman, it has been some time since I attended here and I notice that a number of the faces have changed but, in any case, you will recall that in October of last year I had the pleasure of inviting everybody and hosting a social gathering.

I know that almost everybody in this room lives elsewhere and so I hosted what was billed as the first semi-annual social gathering that had to do with this hearing, and although I am not attending here regularly, I have spoken to some of my fellow counsel over the months and they have all encouraged me to have the second semi-annual social gathering, and so I am doing that.

I am doing it next Wednesday evening, same place, and I believe that at least some of the people who I have had a chance to speak to in the last few days are available, including yourself, sir, and so I am here to - on the record I suppose, or maybe it is off the record - invite everybody and I hope everybody can come.

Thank you.

THE CHAIRMAN: Thank you very much. We will be pleased to attend.

I note that you refer to this as the semi-annual one. We should perhaps be concerned at this time next year if we are into our fourth or fifth semi-annual one. But at this point we are delighted.

Thank you.

All right, ladies and gentlemen, we will adjourn until 1:00 p.m. next Monday.

Thank you.

---Whereupon the hearing adjourned at 12:20 p.m., to be reconvened on Monday, August 14th, 1989, commencing at 1:00 p.m.